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THE FUTURE OF ASTRONOMY[1]

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IT is claimed by astronomers that their science is not only the oldest, but that it is the most highly developed of the sciences. Indeed it should be so, since no other science has ever received such support from royalty, from the state and from the private individual. However this may be, there is no doubt that in recent years astronomers have had granted to them greater opportunities for carrying on large pieces of work than have been entrusted to men in any other department of pure science. One might expect that the practical results of a science like physics would appeal to the man who has made a vast fortune through some of its applications. The telephone, the electric transmission of power, wireless telegraphy and the submarine cable are instances of immense financial returns derived from the most abstruse principles of physics. Yet there are scarcely any physical laboratories devoted to research, or endowed with independent funds for this object, except those supported by the government. The endowment of astronomical observatories devoted to research, and not including that given for teaching, is estimated to amount to half a million dollars annually. Several of the larger observatories have an annual income of fifty thousand dollars.

I once asked the wisest man I know, what was the reason for this difference. He said that it was probably because astronomy appealed to the imagination. A practical man, who has spent all his life in his counting room or mill, is sometimes deeply impressed with the vast

[1] Commencement address at Case School of Applied Science, Cleveland, May 27, 1909.

distances and grandeur of the problems of astronomy, and the very remoteness and difficulty of studying the stars attract him.

My object in calling your attention to this matter is the hope that what I have to say of the organization of astronomy may prove of use to those interested in other branches of science, and that it may lead to placing them on the footing they should hold. My arguments apply with almost equal force to physics, to chemistry, and in fact to almost every branch of physical or natural science, in which knowledge may be advanced by observation or experiment.

The practical value of astronomy in the past is easily established. Without it, international commerce on a large scale would have been impossible. Without the aid of astronomy, accurate boundaries of large tracts of land could not have been defined and standard time would have been impossible. The work of the early astronomers was eminently practical, and appealed at once to every one. This work has now been finished. We can compute the positions of the stars for years, almost for centuries, with all the accuracy needed for navigation, for determining time or for approximate boundaries of countries. The investigations now in progress at the greatest observatories have little, if any, value in dollars and cents. They appeal, however, to the far higher sense, the desire of the intellectual human being to determine the laws of nature, the construction of the material universe, and the properties of the heavenly bodies of which those known to exist far outnumber those that can be seen.

Three great advances have been made in astronomy. First, the invention of the telescope, with which we commonly associate the name of Galileo, from the wonderful results he obtained with it. At that time there was practically no science in America, and for more than two centuries we failed to add materially to this invention. Half a century ago the genius of the members of one family, Alvan Clark and his two sons, placed America in the front rank not only in the construction, but in the possession, of the largest and most perfect telescopes ever made. It is not easy to secure the world's record in any subject. The Clarks constructed successively, the 18-inch lens for Chicago, the 26-inch for Washington, the 30-inch for Pulkowa, the 36-inch for Lick and the 40-inch for Yerkes. Each in turn was the largest yet made, and each time the Clarks were called upon to surpass the world's record, which they themselves had already established. Have we at length reached the limit in size? If we include reflectors, no, since we have mirrors of 60 inches aperture at Mt. Wilson and Cambridge, and a still larger one of 100 inches has been undertaken. It is more than doubtful, however, whether a further increase in size is a great advantage. Much more depends on other conditions, especially those of climate, the kind of work to be done and, more than all,

the man behind the gun. The case is not unlike that of a battleship. Would a ship a thousand feet long always sink one of five hundred feet? It seems as if we had nearly reached the limit of size of telescopes, and as if we must hope for the next improvement in some other direction.

The second great advance in astronomy originated in America, and was in an entirely different direction, the application of photography to the study of the stars. The first photographic image of a star was obtained in 1850, by George P. Bond, with the assistance of Mr. J. A. Whipple, at the Harvard College Observatory. A daguerreotype plate was placed at the focus of the 15-inch equatorial, at that time one of the two largest refracting telescopes in the world. An image of α Lyræ was thus obtained, and for this Mr. Bond received a gold medal at the first international exhibition, that at the Crystal Palace, in London, in 1851. In 1857, Mr. Bond, then Professor Bond, director of the Harvard Observatory, again took up the matter with collodion wet plates, and in three masterly papers showed the advantages of photography in many ways. The lack of sensitiveness of the wet plate was perhaps the only reason why its use progressed but slowly. Quarter of a century later, with the introduction of the dry plate and the gelatine film, a new start was made. These photographic plates were very sensitive, were easily handled, and indefinitely long exposures could be made with them. As a result, photography has superseded visual observations, in many departments of astronomy, and is now carrying them far beyond the limits that would have been deemed possible a few years ago.

The third great advance in astronomy is in photographing the spectra of the stars. The first photograph showing the lines in a stellar spectrum was obtained by Dr. Henry Draper, of New York, in 1872. Sir William Huggins in 1863 had obtained an image of the spectrum of Sirius, on a photographic plate, but no lines were visible in it. In 1876 he again took up the subject, and, by an early publication, preceded Dr. Draper. When we consider the attention the photography of stellar spectra is receiving at the present time, in nearly all the great observatories in the world, it may well be regarded as the third great advance in astronomy.

What will be the fourth advance, and how will it be brought about? To answer this question we must consider the various ways in which astronomy, and for that matter any other science, may be advanced.

First, by educating astronomers. There are many observatories where excellent instruction in astronomy is given, either to the general student or to one who wishes to make it his profession. At almost any active observatory a student would be received as a volunteer assistant. Unfortunately, few young men can afford to accept an unpaid position, and the establishment of a number of fellowships each offering a small salary sufficient to support the student would enable him to acquire the

necessary knowledge to fill a permanent position. The number of these scholarships should not be large, lest more students should undertake the work than would be required to fill the permanent paying positions in astronomy, as they become vacant.

In Europe, a favorite method of aiding science is to offer a prize for the best memoir on a specified subject. On theoretical grounds this is extremely objectionable. Since the papers presented are anonymous and confidential, no one but the judges know how great is the effort wasted in duplication. The larger the prize, the greater the injury to science, since the greater will be the energy diverted from untried fields. It would be much wiser to invite applications, select the man most likely to produce a useful memoir, and award the prize to him if he achieved success.

The award of a medal, if of great intrinsic value, would be an unwise expenditure. The Victoria Cross is an example of a successful foundation, highly prized, but of small intrinsic value. If made of gold, it would carry no greater honor, and would be more liable to be stolen, melted down or pawned.

Honorary membership in a famous society, or honorary degrees, have great value if wisely awarded. Both are highly prized, form an excellent stimulus to continued work, and as they are both priceless, and without price, they in no way diminish the capacity for work. I recently had occasion to compare the progress in various sciences of different countries, and found that the number of persons elected as foreign associates of the seven great national societies of the world was an excellent test. Eighty-seven persons were members of two or more of these societies. Only six are residents of the United States, while an equal number come from Saxony, which has only a twentieth of the population. Of the six residents here, only three were born in the United States. Not a single mathematician, or doctor, from this country appears on the list. Only in astronomy are we well represented. Out of a total of ten astronomers, four come from England, and three from the United States. Comparing the results for the last one hundred and fifty years, we find an extraordinary growth for the German races, an equally surprising diminution for the French and other Latin races, while the proportion of Englishmen has remained unchanged.

A popular method of expending money, both by countries and by individuals, is in sending expeditions to observe solar eclipses. These appeal both to donors and recipients. The former believe that they are making a great contribution to science, while the latter enjoy a long voyage to a distant country, and in case of clouds they are not expected to make any scientific return. If the sky is clear at the time of the eclipse, the newspapers of the next day report that great results have

been secured, and after that nothing further is ever heard. Exceptions should be made of the English Eclipse Committee and the Lick Observatory, which, by long continued study and observation, are gradually solving the difficult problems which can be reached in this way only.

The gift of a large telescope to a university is of very doubtful value, unless it is accompanied, first, by a sum much greater than its cost, necessary to keep it employed in useful work, and secondly, to require that it shall be erected, not on the university grounds, but in some region, probably mountainous or desert, where results of real value can be obtained.

Having thus considered, among others, some of the ways in which astronomy is not likely to be much advanced, we proceed to those which will secure the greatest scientific return for the outlay. One of the best of these is to create a fund to be used in advancing research, subject only to the condition that results of the greatest possible value to science shall be secured. One advantage of this method is that excellent results may be obtained at once from a sum, either large or small. Whatever is at first given may later be increased indefinitely, if the results justify it. One of the wisest as well as the greatest of donors has said: "Find the particular man," but unfortunately, this plan has been actually tried only with some of the smaller funds. Any one who will read the list of researches aided by the Rumford Fund, the Elizabeth Thompson Fund or the Bruce Fund of 1890 will see that the returns are out of all proportion to the money expended. The trustees of such a fund as is here proposed should not regard themselves as patrons conferring a favor on those to whom grants are made, but as men seeking for the means of securing large scientific returns for the money entrusted to them. An astronomer who would aid them in this work, by properly expending a grant, would confer rather than receive a favor. They should search for astronomical bargains, and should try to purchase results where the money could be expended to the best advantage. They should make it their business to learn of the work of every astronomer engaged in original research. A young man who presented a paper of unusual importance at a scientific meeting, or published it in an astronomical journal, would receive a letter inviting him to submit plans to the trustees, if he desired aid in extending his work. In many cases, it would be found that, after working for years under most unfavorable conditions, he had developed a method of great value and had applied it to a few stars, but must now stop for want of means. A small appropriation would enable him to employ an assistant who, in a short time, could do equally good work. The application of this method to a hundred or a thousand stars would then be only a matter of time and money.

The American Astronomical Society met last August at a summer

resort on Lake Erie. About thirty astronomers read papers, and in a large portion of the cases the appropriation of a few hundred dollars would have permitted a great extension in these researches. A sad case is that of a brilliant student who may graduate at a college, take a doctor's degree in astronomy, and perhaps pass a year or two in study at a foreign observatory. He then returns to this country, enthusiastic and full of ideas, and considers himself fortunate in securing a position as astronomer in a little country college. He now finds himself overwhelmed with work as a teacher, without time or appliances for original work. What is worse, no one sympathizes with him in his aspirations, and after a few years he abandons hope and settles down to the dull routine of lectures, recitations and examinations. A little encouragement at the right time, aid by offering to pay for an assistant, for a suitable instrument, or for publishing results, and perhaps a word to the president of his college if the man showed real genius, might make a great astronomer, instead of a poor teacher. For several years, a small fund, yielding a few hundred dollars annually, has been disbursed at Harvard in this way, with very encouraging results.

A second method of aiding astronomy is through the large observatories. These institutions, if properly managed, have after years of careful study and trial developed elaborate systems of solving the great problems of the celestial universe. They are like great factories, which by taking elaborate precautions to save waste at every point, and by improving in every detail both processes and products, are at length obtaining results on a large scale with a perfection and economy far greater than is possible by individuals, or smaller institutions. The expenses of such an observatory are very large, and it has no pecuniary return, since astronomical products are not salable. A great portion of the original endowment has been spent on the plant, expensive buildings and instruments. Current expenditures, like library expenses, heating, lighting, etc., are independent of the output. It is like a man swimming up stream. He may struggle desperately, and yet make no progress. Any gain in power effects a real advance. This is the condition of nearly all the larger observatories. Their income is mainly used for current expenses, which would be nearly the same whatever their output. A relatively small increase in income can thus be spent to great advantage. The principal instruments are rarely used to their full capacities, and the methods employed could be greatly extended without any addition to the executive or other similar expenses. A man superintending the work of several assistants can often have their number doubled, and his output increased in nearly the same proportion, with no additional expense except the moderate one of their salaries. A single observatory could thus easily do double the work that could be accomplished if its resources were divided between two of half the size.

A third, and perhaps the best, method of making a real advance in astronomy is by securing the united work of the leading astronomers of the world. The best example of this is the work undertaken in 1870 by the Astronomische Gesellschaft, the great astronomical society of the world. The sky was divided into zones, and astronomers were invited to measure the positions of all the stars in these zones. The observation of two of the northern and two of the southern zones were undertaken by American observatories. The zone from $+1°$ to $+5°$ was undertaken by the Chicago Observatory, but was abandoned owing to the great fire of 1871, and the work was assumed and carried to completion by the Dudley Observatory at Albany. The zone from $+50°$ to $+55°$ was undertaken by Harvard. An observer and corps of assistants worked on this problem for a quarter of a century. The completed results now fill seven quarto volumes of our annals. Of the southern zones, that from $-14°$ to $-18°$ was undertaken by the Naval Observatory at Washington, and is now finished. The zone from $-10°$ to $-14°$ was undertaken at Harvard, and a second observer and corps of assistants have been working on it for twenty years. It is now nearly completed, and we hope to begin its publication this year. The other zones were taken by European astronomers. As a result of the whole, we have the precise positions of nearly a hundred and fifty thousand stars, which serve as a basis for the places of all the objects in the sky.

Another example of cooperative work is a plan proposed by the writer in 1906, at the celebration of the two-hundredth anniversary of the birth of Franklin. It was proposed, first to find the best place in the world for an astronomical observatory, which would probably be in South Africa, to erect there a telescope of the largest size, a reflector of seven feet aperture. This instrument should be kept at work throughout every clear night, taking photographs according to a plan recommended by an international committee of astronomers. The resulting plates should not be regarded as belonging to a single institution, but should be at the service of whoever could make the best use of them. Copies of any, or all, would be furnished at cost to any one who wished for them. As an example of their use, suppose that an astronomer at a little German University should discover a law regulating the stars in clusters. Perhaps he has only a small telescope, near the smoke and haze of a large city, and has no means of securing the photographs he needs. He would apply to the committee, and they would vote that ten photographs of twenty clusters, each with an exposure of an hour, should be taken with the large telescope. This would occupy about a tenth part of the time of the telescope for a year. After making copies, the photographs would be sent to the astronomer who would perhaps spend ten years in studying and measuring them. The committee

would have funds at their disposal to furnish him, if necessary, with suitable measuring instruments, assistants for reducing the results, and means for publication. They would thus obtain the services of the most skilful living astronomers, each in his own special line of work, and the latter would obtain in their own homes material for study, the best that the world could supply. Undoubtedly, by such a combination if properly organized, results could be obtained far better than is now possible by the best individual work, and at a relatively small expense. Many years of preparation will evidently be needed to carry out such a plan, and to save time we have taken the first step and have sent a skilful and experienced observer to South Africa to study its climate and compare it with the experience he has gained during the last twenty years from a similar study of the climate of South America and the western portion of the United States.

The next question to be considered is in what direction we may expect the greatest advance in astronomy will be made. Fortunate indeed would be the astronomer who could answer this question correctly. When Ptolemy made the first catalogue of the stars, he little expected that his observations would have any value nearly two thousand years later. The alchemists had no reason to doubt that their results were as important as those of the chemists. The astrologers were respected as much as the astronomers. Although there is a certain amount of fashion in astronomy, yet perhaps the best test is the judgment of those who have devoted their lives to that science. Thirty years ago the field was narrow. It was the era of big telescopes. Every astronomer wanted a larger telescope than his neighbors, with which to measure double stars. If he could not get such an instrument, he measured the positions of the stars with a transit circle. Then came astrophysics, including photography, spectroscopy and photometry. The study of the motion of the stars along the line of sight, by means of photographs of their spectra, is now the favorite investigation at nearly all the great observatories of the world. The study of the surfaces of the planets, while the favorite subject with the public, next to the destruction of the earth by a comet, does not seem to appeal to astronomers. Undoubtedly, the only way to advance our knowledge in this direction is by the most powerful instruments, mounted in the best possible locations. Great astronomers are very conservative, and any sensational story in the newspapers is likely to have but little support from them. Instead of aiding, it greatly injures real progress in science.

There is no doubt that, during the next half century, much time and energy will be devoted to the study of the fixed stars. The study of their motions as indicated by their change in position was pursued with great care by the older astronomers. The apparent motions were so

small that a long series of years was required and, in general, for want of early observations of the precise positions of the faint stars, this work was confined mainly to the bright stars. Photography is yearly adding a vast amount of material available for this study, but the minuteness of the quantities to be measured renders an accurate determination of their laws very difficult. Moreover, we can thus only determine the motions at right angles to the line of sight, the motion towards us or from us being entirely insensible in this way. Then came the discovery of the change in the spectrum when a body was in motion, but still this change was so small that visual observations of it proved of but little value. Attaching a carefully constructed spectroscope to one of the great telescopes of the world, photographing the spectrum of a star, and measuring it with the greatest care, provided a tool of wonderful efficiency. The motion, which sometimes amounts to several hundreds of miles a second could thus be measured to within a fraction of a mile. The discovery that the motion was variable, owing to the star's revolving around a great dark planet sometimes larger than the star, added greatly not only to the interest of these researches, but also to the labor involved. Instead of a single measure for each star, in the case of the so-called spectroscopic binaries, we must make enough measures to determine the dimensions of the orbit, its form and the period of revolution.

What has been said of the motions of the stars applies also, in general, to the determination of their distances. A vast amount of labor has been expended on this problem. When at length the distance of a single star was finally determined, the quantity to be measured was so small as to be nearly concealed by the unavoidable errors of measurement. The parallax, or one half of the change in the apparent position of the stars as the earth moves around the sun, has its largest value for the nearest stars. No case has yet been found in which this quantity is as large as a foot rule seen at a distance of fifty miles, and for comparatively few stars is it certainly appreciable. An extraordinary degree of precision has been attained in recent measures of this quantity, but for a really satisfactory solution of this problem, we must probably devise some new method, like the use of the spectroscope for determining motions. Two or three illustrations of the kind of methods which might be used to solve this problem may be of interest. There are certain indications of the presence of a selective absorbing medium in space. That is, a medium like red glass, for instance, which would cut off the blue light more than the red light. Such a medium would render the blue end of the spectrum of a distant star much fainter, as compared with the red end, than in the case of a near star. A measure of the relative intensity of the two rays would serve to measure the distance, or thickness of the absorbing medium. The effect would be the

same for all stars of the same class of spectrum. It could be tested by the stars forming a cluster, like the Pleiades, which are doubtless all at nearly the same distance from us. The spectra of stars of the tenth magnitude, or fainter, can be photographed well enough to be measured in this way, so that the relative distances of nearly a million stars could be thus determined.

Another method which would have a more limited application, would depend on the velocity of light. It has been maintained that the velocity of light in space is not the same for different colors. Certain stars, called Algol stars, vary in light at regular intervals when partially eclipsed by the interposition of a large dark satellite. Recent observations of these eclipses, through glass of different colors, show variations in the time of obscuration. Apparently, some of the rays reach the earth sooner than others, although all leave the star at the same time. As the entire time may amount to several centuries, an excessively small difference in velocity would be recognizable. A more delicate test would be to measure the intensity of different portions of the spectrum at a time when the light is changing most rapidly. The effect should be opposite according as the light is increasing or diminishing. It should also show itself in the measures of all spectroscopic binaries.

A third method of great promise depends on a remarkable investigation carried on in the physical laboratory of the Case School of Applied Science. According to the undulatory theory of light, all space is filled with a medium called ether, like air, but as much more tenuous than air as air is more tenuous than the densest metals. As the earth is moving through space at the rate of several miles a second, we should expect to feel a breeze as we rush through the ether, like that of the air when in an automobile we are moving with but one thousandth part of this velocity. The problem is one of the greatest delicacy, but a former officer of the Case School, one of the most eminent of living physicists, devised a method of solving it. The extraordinary result was reached that no breeze was perceptible. This result appeared to be so improbable that it has been tested again and again, but every time, the more delicate the instrument employed, the more certainly is the law established. If we could determine our motion with reference to the ether, we should have a fixed line of reference to which all other motions could be referred. This would give us a line of ever-increasing length from which to measure stellar distances.

Still another method depends on the motion of the sun in space. There is some evidence that this motion is not straight, but along a curved line. We see the stars, not as they are now, but as they were when the light left them. In the case of the distant stars this may have occurred centuries ago. Accordingly, if we measure the motion of the sun from them, and from near stars, a comparison with its actual mo-

tion will give us a clue to their distances. Unfortunately, all the stars appear to have large motions whose law we do not know, and therefore we have no definite starting point unless we can refer all to the ether which may be assumed to be at rest.

If the views expressed to you this morning are correct, we may expect that the future of astronomy will take the following form: There will be at least one very large observatory employing one or two hundred assistants, and maintaining three stations. Two of these will be observing stations, one in the western part of the United States, not far from latitude + 30°, the other similarly situated in the southern hemisphere, probably in South Africa, in latitude — 30°. The locations will be selected wholly from their climatic conditions. They will be moderately high, from five to ten thousand feet, and in desert regions. The altitude will prevent extreme heat, and clouds or rain will be rare. The range of temperature and unsteadiness of the air will be diminished by placing them on hills a few hundred feet above the surrounding country. The equipment and work of the two stations will be substantially the same. Each will have telescopes and other instruments of the largest size, which will be kept at work throughout the whole of every clear night. The observers will do but little work in the daytime, except perhaps on the sun, and will not undertake much of the computation or reductions. This last work will be carried on at a third station, which will be near a large city where the cost of living and of intellectual labor is low. The photographs will be measured and stored at this station, and all the results will be prepared for publication, and printed there. The work of all three stations will be carefully organized so as to obtain the greatest result for a given expenditure. Every inducement will be offered to visiting astronomers who wish to do serious work at either of the stations and also to students who intend to make astronomy their profession. In the case of photographic investigations it will be best to send the photographs so that astronomers desiring them can work at home. The work of the young astronomers throughout the world will be watched carefully and large appropriations made to them if it appears that they can spend them to advantage. Similar aid will be rendered to astronomers engaged in teaching, and to any one, professional or amateur, capable of doing work of the highest grade. As a fundamental condition for success, no restrictions will be made that will interfere with the greatest scientific efficiency, and no personal or local prejudices that will restrict the work.

These plans may seem to you visionary, and too Utopian for the twentieth century. But they may be nearer fulfilment than we anticipate. The true astronomer of to-day is eminently a practical man. He does not accept plans of a sensational character. The same qualities

are needed in directing a great observatory successfully, as in managing a railroad, or factory. Any one can propose a gigantic expenditure, but to prove to a shrewd man of affairs that it is feasible and advisable is a very different matter. It is much more difficult to give away money wisely than to earn it. Many men have made great fortunes, but few have learned how to expend money wisely in advancing science, or to give it away judiciously. Many persons have given large sums to astronomy, and some day we shall find the man with broad views who will decide to have the advice and aid of the astronomers of the world, in his plans for promoting science, and who will thus expend his money, as he made it, taking the greatest care that not one dollar is wasted. Again, let us consider the next great advance, which perhaps will be a method of determining the distances of the stars. Many of us are working on this problem, the solution of which may come to some one any day. The present field is a wide one, the prospects are now very bright, and we may look forward to as great an advance in the twentieth century, as in the nineteenth. May a portion of this come to the Case School and, with your support, may its enviable record, in the past, be surpassed by its future achievements.

THE FUTURE OF MATHEMATICS

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PROFESSOR A. VOSS, of the University of Munich, recently made the following statement: "Our entire present civilization, as far as it depends upon the intellectual penetration and utilization of nature, has its real foundation in the mathematical sciences."[1] He adds that this truth finds expression in the ever-increasing appreciation of the educational value of mathematics, notwithstanding the fact that it is the most unpopular of all the sciences. This unpopularity is natural since "unpopularity is an essential feature of a real science," because such a science can be comprehended only through tireless and continued efforts.

An intelligent expression as regards the future of mathematics must be based not only upon the past and present state of this science, but also upon its real essence. One of those elements which mathematics has in common with some of the other sciences, but which are more prominent in mathematics than in any of the others, is the tendency to use thought in the most economical manner. When one considers the extent to which efforts to simplify methods, theorems and formulas direct mathematical endeavor, one must admit that the statement "Mathematics is the science of saving thought" expresses a great truth, even if it is too sweeping to serve as a definition.

That mathematics is the science which is preeminently devoted to the discovery and mapping of routes along which thought may ascend securely and with the greatest ease, is supported by the fact that it has the oldest and the most extensive symbolical language. In the introduction to his classic history of mathematics, Moritz Cantor asks, "Why has mathematics, since the remotest times, found support, simplification and advancement by means of word symbols, whether these are number symbols or other mathematical symbols?" Although the oldest of these word symbols are probably relics of a very ancient picture language, yet it is of great interest that in mathematics the picture language was retained and used side by side with an alphabetic and syllabic language, while the latter displaced the former elsewhere. Even those who have mastered only the elements of algebra and the

[1] Voss, "Ueber das Wesen der Mathematik." Rede gehalten om 11. Marz, 1908, in der oeffentlichen Sitzung der k. bayerischen Akademie der Wissenschaften; Teubner, 1908, p. 4.

differential calculus are in position to appreciate the value of mathematical symbols for the purpose of centralizing and intensifying thought.

It is true that some of the roads which mathematical thought has made through great difficulties have been practically abandoned and that the popularity of many of the others has changed from time to time. Among the former we may class the results of investigations recorded at the beginning of the oldest extensive mathematical work that has been deciphered, viz., the formulas relating to unit fractions which are found in the nearly four thousand-years-old work of Ahmes. A subject which appears to have been placed at the very beginning of advanced mathematical instruction four thousand years ago is now entirely abandoned in our courses, except when the history of the development of the science is under consideration. While mathematics presents a number of other roads which are now of interest only to the historian, yet there are also many which have been known for centuries and which have been pursued with profit and pleasure by great minds in all the civilized nations. The latter class includes all the longer ones leading gradually to points of view from which the connection between many natural phenomena may be clearly discerned.

The intellectual heights reached by means of a long series of connected mathematical theorems do not always reveal their greatest lesson to the first explorers. For instance, the large body of facts relating to conic sections, developed by Apollonius and other Greek geometers, became a much greater glory to the human mind through the discovery, nearly two thousand years later, that the bodies of the solar system describe conic sections. Such experiences in the past tend to justify the fact that a large number of men are devoting their lives to the discovery of abstract results irrespective of applications, and they tend to explain why the largest prize (about twenty-five thousand dollars) ever offered for a mathematical theorem is being offered for a theorem in number theory, which is not expected to have any application to subjects outside of pure mathematics.

There seems to be a general impression abroad to the effect that mathematics and the ancient languages constituted the main parts of the curriculums of our colleges and universities a century or two ago. As regards mathematics this is quite contrary to fact, as may be seen from a few historical data. Less than two centuries ago the students in Harvard College began the study of arithmetic in their senior year. In fact, no knowledge of any mathematics was required to enter Harvard before 1803, and it was not until 1816 that the whole of arithmetic was required for entrance. In other American institutions the mathematical situation was generally worse, and in Europe the improvements were not very much earlier. It is during compara-

tively recent years that mathematics has made most of its gains towards being recognized as a fundamental science, and the study of advanced mathematics in our universities had a still later origin.

The rapid recent advances in various fields of mathematics have given rise to a very optimistic spirit as to the future. Although we still hold in high esteem the brilliant discoveries of the Greeks, we are inclined to give much more thought and attention to recent work, as may be seen from the references in the extensive German and French mathematical encyclopedias which are in the process of being published. The history of mathematics furnishes many instances of the vanishing of apparently insurmountable barriers. We need only recall the barrier created by the Greek custom of confining oneself to the rule and circle in the most acceptable geometric constructions, and the very formidable barrier furnished by the imaginary, and even by the negative and the irrational roots of a quadratic equation.

Those who fixed their attention upon these barriers in the past have naturally been led to think that the days of important advances in mathematics were about ended and that it only remained to fill in details. Such predictions had few supporters when new methods led over these barrier and turned them into steps to richer mathematical domains. As this process has been repeated so often it has gradually reduced the number of those to whom the future of mathematics looked dark. In fact, Poincaré, in his address[2] before the Fourth International Congress of Mathematicians, which was held at Rome, in April, 1908, said that all those who held these views are dead.

These facts seem to justify a very hopeful spirit as regards future progress, but it is necessary to examine them with great care in order to deduce from them any helpful suggestions as to the probable nature of this progress. Such prognostications clearly demand a mind that can deal with big problems as well as a thorough acquaintance with the past and the present developments in mathematics, to insure that the results obtained by a kind of extrapolation may be worthy of confidence. It is doubtful whether any living mathematician would be more generally regarded as qualified to make reliable predictions along this line than Poincaré, of Paris. The address to which we referred in the preceding paragraph was devoted to this subject and we proceed to give some of the main results.

The objects of mathematical thought are so numerous that we cannot expect to exhaust them. This appears the more evident since the mathematician creates new concepts from the elements which are presented to him by nature. Hence there must be a choice of subject matter, but who is to do the choosing? Some are inclined to think that the mathematician should confine himself to those problems which

[2] *Bulletin des Sciences Mathématiques*, Vol. 32 (1908), p. 168.

may be set for him by the physicist or the engineer. If he had done this in the past he would not have created the instruments necessary to solve such problems, and hence it is unreasonable to make such restrictions as to the future.

If the physicists of the eighteenth century had abandoned the study of electricity because it seemed to serve no useful end, we should not have had the many useful applications of electricity during the nineteenth century. Similarly, if the mathematician had abandoned the study of negative and imaginary numbers because they seemed to point only to impossibilities, we should not have had the many powerful instruments of thought which enable us to cope more successfully with many problems of nature. Just as the physicist is largely guided in his work by those facts which seem to point to general laws, so the mathematician is guided in his work by the desire to discover extensive relations and laws having a wide range of application. Millions of isolated facts present themselves to the investigator, some of which are of striking interest to the initiated, but they are of practically no value in the development of mathematics except that they may sometimes serve as an exercise in secondary instruction.

At a first thought the statement that "Mathematics is the art of giving the same name to different things" may appear to be entirely contrary to fact, but from a certain standpoint this statement conveys a very fundamental truth. It should be borne in mind that these different things must have in common the property to which this common name refers, and that it is the duty of the mathematician to discover and exhibit this common property. By way of illustration we may recall the use of x for various unknowns in algebra and the (1, 1) correspondence between the two series of operators. When the language has been properly chosen it is often surprising to find that the demonstrations, as regards a known object, apply immediately to a large number of new objects without even a change of name.

Just as the boundaries between the elementary subjects of mathematics—arithmetic, algebra and geometry—vanish when the knowledge of these subjects is sufficiently extended, so the boundaries between subjects in pure and applied mathematics are disappearing, and it is exactly in these bordered lands, or in this common territory of two or more subjects, where the greatest recent progress has been made and where the greatest future activity may be expected. The work in this common territory is made possible by observing similarity of form where there is dissimilarity of matter, or by observing some other common properties which admit mathematical treatment.

In Poincaré's address some of these general observations were illustrated by numerous examples chosen from various fields of higher mathematics. On the contrary, we shall confine our illustrative ex-

amples to elementary subjects. Our first effort will be directed towards exhibiting some territory which is common to each of the four subjects —arithemtic, geometry, algebra and trigonometry. By observing common properties we shall not only see a bond connecting these fundamental subjects, but we shall also be led to general methods which make it unnecessary to study the same properties in different forms. The thing to be emphasized is that these four elementary subjects have in common fundamental notions which not only connect them, but also establish contact between them and many other subjects. Such a fundamental notion is a group of order 8, known as the *octic group*. Some of the properties of this group may be easily seen by considering the possible movements of space which transform a square into itself.

The period or order of a movement represents the number of times the movement must be made in order to arrive at the identity, or at the original position. It is clear that the eight movements of the square include two of period four, five of period two, and the identity A profound study of these eight movements would disclose many interesting facts. For instance, it would be seen that only two of them (the square of these of period four and the identity) are commutative with each one of others, while each one of the remaining six is commutative with only four of the possible eight movements. Although a profound study of this group of eight movements would be necessary to exhibit the fundamental rôle which it plays in the various subjects, it is not necessary to enter deeply into its properties in order to see that it is common to the four subjects mentioned above.

At a first thought it might appear as if these eight movements had nothing in common with trigonometry, but a very fundamental connection may be seen as follows: If the vertex of the angle A is the center of a square and the initial line of A coincides with a line of symmetry of the square, the operations of taking the complement and the supplement of A correspond to movements transforming the square into itself. Hence the eight angles which may be obtained from a given angle by a repetition of finding supplement and complement may be placed in a (1, 1) correspondence with the eight movements of the square. As these eight angles play such a fundamental rôle in elementary trigonometry, it has been suggested that our ordinary school trigonometry might appropriately be called the trigonometry of the octic group, or the trigonometry of the group of movements of the square.

Although the eight operations of the octic group do not occupy such an important place in elementary arithmetic as in geometry and trigonometry, yet these operations serve to explain some facts which present themselves in the most elementary arithmetic processes. For instance, the operations of subtracting from 2 and dividing 2 lead, in

general, to eight distinct numbers. Starting with 5, these eight numbers are

$$5, -3, \tfrac{2}{5}, -\tfrac{2}{3}, \tfrac{8}{5}, \tfrac{8}{3}, \tfrac{5}{4}, \tfrac{3}{4}.$$

No new number is obtained by dividing 2 by any of these numbers or by subtracting any of them from 2. The proof of the fact that the eight operations by means of which each one of these eight numbers may be derived from any one of them have the same properties in relation to each other as the eight movements of the square is not difficult, but it involves details which may be omitted in a popular exposition.

An instance where the octic group plays an important rôle in algebra is furnished by the three-valued function $xy + zw$, which is fundamental in the theory of the general equation of the fourth degree. On account of the existence of this function the solution of the general equation of the fourth degree may be made to depend upon the solution of the general equation of the third degree. This function is transformed into itself by eight substitutions, and we may arrange its letters separately on the vertices of a square in such a way that the eight substitutions transforming the function into itself correspond to the eight movements which transform the square into itself. Such an arrangement exhibits the intimate relations between this function and the movements of a square, and the preceding examples illustrate the fact that the octic group finds application in each of the elementary subjects—arithmetic, algebra, geometry and trigonometry, and that it forms a part of the domain common to all of these disciplines.

In a similar manner other groups could be traced through these elementary subjects of mathematics and it could be shown that the theory of these groups may be used to clarify many fundamental points and to exhibit deep-seated contact. If the common domains will furnish the most active fields of future investigations in accord with the predictions of Poincaré, and if we may expect the greatest future progress to be based upon the modeling of the less advanced science upon the one which has made the more progress, it is reasonable to expect that a subject like group theory will grow in favor, and that some of the elements of this subject will become a part of the ordinary courses in secondary mathematics. In support of this view we may quote a recent statement by Professor Bryan, President of the Mathematical Association, which is as follows: "I believe Professor Perry will get some very good material for applications out of the theory of groups, when explorers have first made their discoveries, and when the colonists have been over it and surveyed it, and discovered means for cultivating it. We do not know anything about its practical applications now."[3]

[3] *The Mathematical Gazette*, January, 1909, p. 17.

The future of mathematics appears bright, both for the investigator and for the teacher. When a country which has such an enlightened educational system as France increased the amount of time devoted to secondary mathematics so recently as 1902 and again in 1906, it furnishes one of the strongest possible encouragements to the teacher who may have been troubled by the thought that the educational value of mathematics was not being as fully appreciated as in earlier years. Naturally we may expect that there will be local changes of view as regards the value of mathematics as an educational subject, and these changes will not always be for the better, but the civilized world, as a whole, is learning to appreciate more and more the fundamental importance of early mathematical training, so that we should not be too much perturbed by local steps backwards, but we should move ahead with the assurance that we are engaged in a work of the highest pedagogical importance.

The boundless confidence in the importance of early and extensive mathematical training should, however, not blind us to the need of changes and new adaptations. As an important function of mathematical training is the furnishing of the most useful and the most powerful tools of thought, it is evident that the choice of these tools will vary with the advancement of general knowledge. All admit that the concept of a derivative is one of the most useful elementary tools of thought, and in a number of countries this concept has been introduced into secondary mathematics and used with success. At the last International Mathematical Congress, held at Rome, M. Borel, of Paris, reported that the notion of derivative had been introduced into French secondary education in 1902 and that it had led to satisfactory results. At the same meeting M. Beke, of Budapest, stated that this notion, together with the notion of function and graph, had been introduced into the courses of secondary education in Hungary.

At the recent joint conference of the Mathematical Association and the Federated Association of London Non-Primary Teachers, the chairman remarked: "I have always thought that a mathematician was a man who when he wants to find anything out, uses his brains for that purpose, whereas a physicist, when he wants to find out anything, resorts to experiment." Although this statement is not to be construed literally, yet it does involve a great partial truth and it calls attention to elements which insure mathematical appreciation as long as there is scientific thought. "It is the mind that sees as well as the eye," and the mind sees some of the greatest truths most clearly by means of mathematical symbolism. In fact, mathematical symbols serve both as a telescope and also as a microscope for mental vision, and as long as such vision is demanded the teacher of mathematics will be appreciated.

THE "DRUID STONES" OF BRITTANY

By Professor J. S. KINGSLEY

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THE writer makes no pretense of being an archeologist, but finding few accounts of the wonderful megalithic monuments of Brittany in English, he has written this account of his visit to them as thread on which to string a few pictures. Those huge stones erected by human hands—no one knows by whom or why or when—which are called megalithic monuments, occur throughout western Europe, from the "Huns' beds" east of the Zuider Zee, through Britain, France and Spain and into northern Africa, across the Strait of Gibraltar, but nowhere are they as numerous or striking as in Brittany. The tourist is familiar with that strange circle of standing stones at Stonehenge and, to a less extent with "Kit's Coty House" in Kent and the circle at Avebury, but Morbihan is far out of the usual track and hence is seen by comparatively few of our people.

The department of Morbihan lies on the southern shore of Brittany, three hundred miles in a straight line west of Paris, and considerably farther as the trains run. The part of it where these megaliths abound is, perhaps, twenty miles, east and west, and ten north and south. It contains no large cities—Vannes, the capital, has not twenty-five thousand inhabitants —it has no churches or art galleries starred in Bädeker; its sole attractions are its delightful inhabitants who still adhere to their ancient costumes, and the monuments.

Archeologists divide these standing stones into different categories, according to the way they are arranged, and each kind has its name derived from either the Keltic or the French. There are *menhirs* (Keltic, long stones) which stand upright in the soil, usually upon the smaller end. Menhirs may be isolated, scattered here and there through the region, or they may be arranged in lines or rows (*alignments*) stretching across the fields. In certain places the menhirs form square or semicircular enclosures called *cromlechs* (Keltic, curved stones).

Again, the megaliths have been built into chambers, the walls composed of upright stones placed close together, and roofed in by one or more large blocks of stone. These are the *dolmens*[1] (table stones), the enlarged chamber being usually reached by a narrower passage, though occasionally the entrance is in one side of the chamber. In some cases

[1] In England the dolmens are frequently called cromlechs.

instead of a true dolmen there is a narrow passage alone, an *allée couverte*. Various subdivisions of these types are recognized, but they may be ignored here.

It was to see these megaliths that we took the all-day journey (really only 148 miles) across Brittany. It was early morning when we left the wonderful rock of St. Michel's Mount and the omelettes of the now reconciled Poulards, with whose quarrels all travelers are familiar. Half an hour by tram took us to Pontorson. Why do places like Pontorson exist? Our two hours were one continual struggle with station agent, hack drivers and porters, all of whom were insistent that we should drive out to Mt. St. Michel. "One franc a person" but we knew their ways; half way there would be a demand for a pourboire which would make the original fare look like twenty cents. Besides, we had just come from the Mount, and why should we go back? At Dol another wait, this time long enough to get an early lunch, before we could get a cross-country train for Rennes. Up to that day Rennes had been associated in our minds with the Dreyfus trial of a few years ago. Two hours here were sufficient to assure us that Bädeker did not slander the town when he wrote that with its 75,000 inhabitants, "its spacious modern streets are generally dull, lifeless and deserted." Next a wait of an hour at Redon before taking the last train of the day—the Nantes-l'Orient express for Auray, which we reached just in time for dinner at the most comfortable and hospitable Pavilion hotel.

One may go in various ways from the railway to the monuments, but there is a best way—by carriage. There is the route from Vannes, taking a boat down through the sea of Morbihan to Locmariaquer. It is a picturesque route through a land-locked arm of the sea, studded with islands, like a miniature Casco Bay. But it is not to be depended upon, as the sailing of the steamers varies with the tides. It is cheaper to take the train from Auray to Carnac station on the little road to Quiberon, and then the little tram to Carnac village and to Erdeven. This brings one within easy walking distance of the principal alignments; but to reach the other monuments a carriage is convenient; even necessary, if one is to see the important menhirs, dolmens, etc., of Locmariaquer. Besides, the foot traveler will have to have a guide, otherwise he will waste much time and probably will miss much that he ought to see. Bädeker's map is on too small a scale to be of much assistance.

The total drive from Auray to all of the standing stones is about thirty-five miles, but by cutting out some which apparently were repetitions of others, we made our round trip about twenty-five miles. The country traversed is best described in the terms of physical geography as a peneplain and the shore to the south as a drowned coast. It is nearly level, with no hills rising markedly above the rest of the country.

We saw the first of the monuments about six miles out of Auray and

our first glimpse was rather disappointing. A couple of hundred feet to the left of the road was the first dolmen, a dozen stones, about five feet high, standing upright in a pasture, and roofed in by two large stones lying across them. It recalled a child's house on a large scale, built out of the lichen-covered stones of the field. By its side stood a square stone monument announcing that this dolmen of Keriaval is the property of the French Republic, and that any one injuring it in any way will be prosecuted. It may be said that by each group in the entire district is a similar stone. On the other side of the road are three dolmens, close together, standing scarcely above the surrounding soil but excavated inside so that one may stand upright in the interior.

It would serve no useful purpose to give our itinerary in detail, but a clearer idea of these strange structures may be given by a general description of the monuments as a whole, specifying here and there those of more particular interest from size or other features.

Possibly the most striking of all are the alignments. Certainly they are the most difficult to explain. Of these there are several groups, each distinct from its fellows, and yet the whole series being in the same belt. Many of the stones have tumbled down and some have been utilized in building walls and houses. Thus the little church of St. Cornély at Carnac is built entirely of menhirs, broken up into blocks of convenient size, while the curious crown that surmounts its west portal was carved out of a single menhir. Le Rouzic, whom I shall often quote, says that the series of alignments once extended from a point to the west of the village of Carnac, five miles east to the Crac'h River, while other series occur further west, near Erdeven.

FIG. 1. DOLMEN OF KÉRIAVAL, half way between Auray and Carnac.

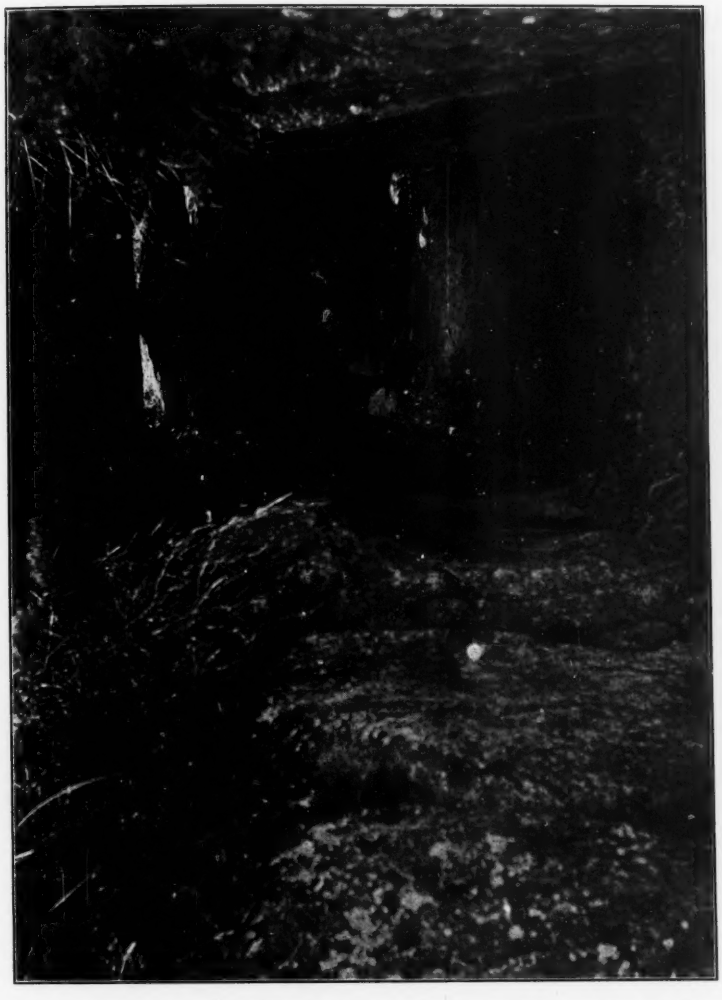

FIG. 2. INTERIOR OF THE DOLMEN OF KERIONED, across the road from Kérlaval.

We visited the three alignments near Carnac—Ménec to the west, Kermario in the middle and Kerlescan to the east. Of these Ménec is the most extensive and the best preserved, but the menhirs in the others are larger. That these alignments are distinct from each other and are not parts of a single one is shown by several facts. They are separated by considerable intervals, the gap betweei Ménec and Kermario being a thousand feet; between Kermario and Kerlescan over a quarter of a mile. Again, the rows in the different alignments run in different directions—Ménec N. 70° E., Kermario N. 57° E., and Kerlescan S. 85° E. In each alignment the rows begin with enormous stones at the west end and gradually taper down to merely good-sized rocks at the easterly ends. Then Ménec and Kerlescan begin at the

FIG. 3. PANORAMA OF THE ALIGNMENT OF MÉNEC, near Carnac, from the westerly end. The cromlech is just behind the position of the camera, but could not be included in the view on account of a house and farm buildings. The alignment of

western ends with a cromlech, and LeRouzic is confident that there was originally a cromlech at the western end of Kermario, but that it has disappeared.

The alignment of Ménec may be taken as typical. It lies in an undulating pasture, with farm buildings here and there, and is crossed at about the middle by a country road. Through this field, from the slight elevation at the west, down through the hollow of the road, and disappearing over the rise at the east, stretch eleven rows of menhirs, the rows being approximately parallel and about thirty feet apart, and the whole a little over three hundred feet wide while in length they extend 3,800 feet, or over two thirds of a mile. Some of the menhirs have tumbled down; here and there we note one built into the walls separating the fields but still occupying its original position. In all there are 1,099 stones still standing in Ménec. At the eastern end they are small, rising but two or three feet above the soil, but at the western end are the giants, three or four feet in diameter and thirteen feet high.

The cromlech of Ménec consists of 70 stones, about five feet high on the average, which sweep in a semicircle around the farm buildings at the west end of the alignment, the chord of the curve including only the southern half of the lines proper.

Only dry facts need be given concerning the other alignments we saw. In Kermario there are 982 menhirs in ten rows, extending over

Kermario is behind the woods at the farther end of the lines. At the extreme right of the view is the Mont St. Michel, a sepulchral monument or galgal, entirely artificial (see p. 134).

an area about 320 by 3,700 feet. The largest menhir, which has fallen, is 21 feet in total length, while the smallest stands but a foot and a half above the ground. In Kerlescan the cromlech of 39 stones is quadrangular in outline with rounded corners, while the alignment proper consists of 540 menhirs in thirteen rows in an area 2,700 feet long with an interruption of 600 feet where the little village of Kerlescan is situated. The largest of the stones is thirteen feet in height, the smallest only two feet above the surface of the ground.

While dolmens and isolated menhirs occur all around Carnac, the most striking of them are on the next peninsula to the east, near the little village of Locmariaquer. Here one must leave the road and go into the fields to see the monuments. Suddenly a one-armed man sprang up beside the carriage and led the way among farm outhouses, gardens and across vegetable patches, to the most remarkable of all these remains, which continually bring up the question, How could they have been erected? Largest of all is the gigantic menhir, "menhir groach," in the village itself, now fallen and broken into five pieces. According to Le Rouzic, who has measured it carefully and who has taken the specific gravity of the stone, it was originally 68 feet in length and weighed 382 tons. Le Rouzic also says that the time and cause of its fall are unknown, and cites a drawing of 1727 to show that at that time it was in its present condition. On the other hand, I have seen a little pam-

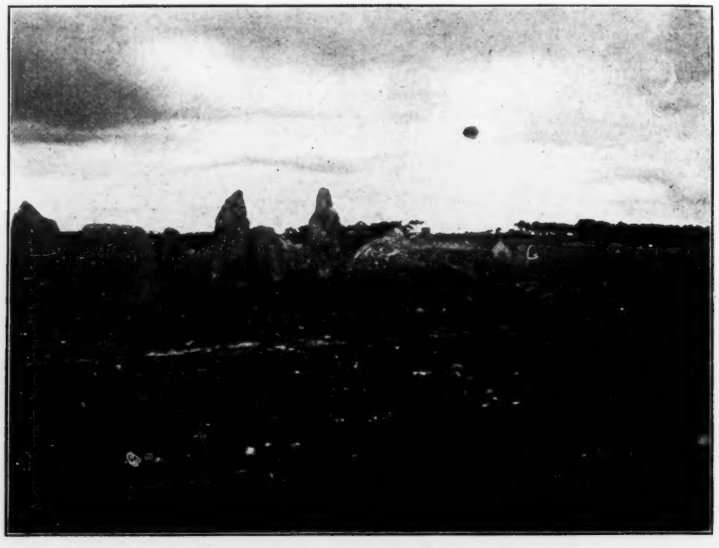

FIG. 4. ALIGNMENT OF KERMARIO.

phlet which states that it was struck by lightning, overthrown and broken in the sixteenth century. There is apparently little doubt that once this immense stone stood vertically, but the engineering problem of its erection is not easily solved. One can hardly believe, with Le Rouzic, that the lever and inclined plane were sufficient; yet what other mechanical aids could have been available?

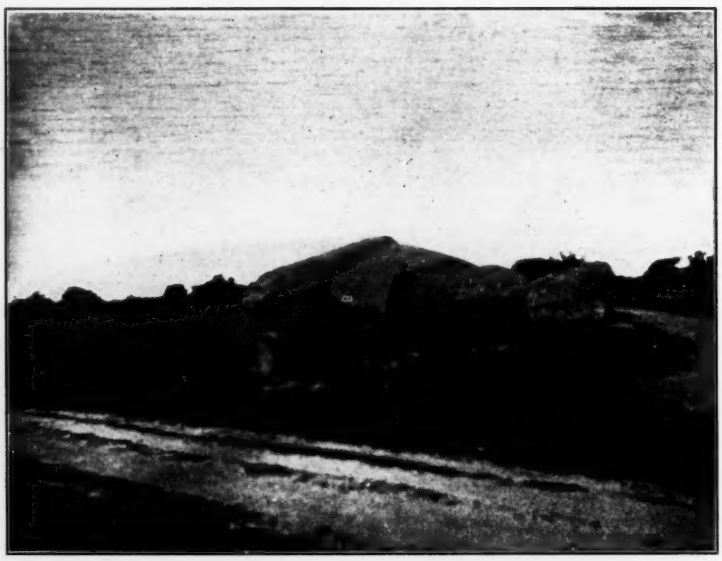

FIG. 5. MENHIR GROACH OR GRAND MENHIR, LOCMARIAQUER.

At Locmariaquer is also the largest of the dolmens, the "dolmen des marchands." I regret that I took no measurements of its size, especially since none are given in the works at hand. I can only depend upon my memory, aided by pictures, for my estimates. At its southern end is a passage about four feet wide and high enough for a tall man to stand erect. This is walled by vertical slabs of stone and roofed in with the same material. The passage leads to a larger chamber which is at least seven feet wide and high by possibly ten or twelve in length. The end opposite the entrance is formed of a single stone, shaped like

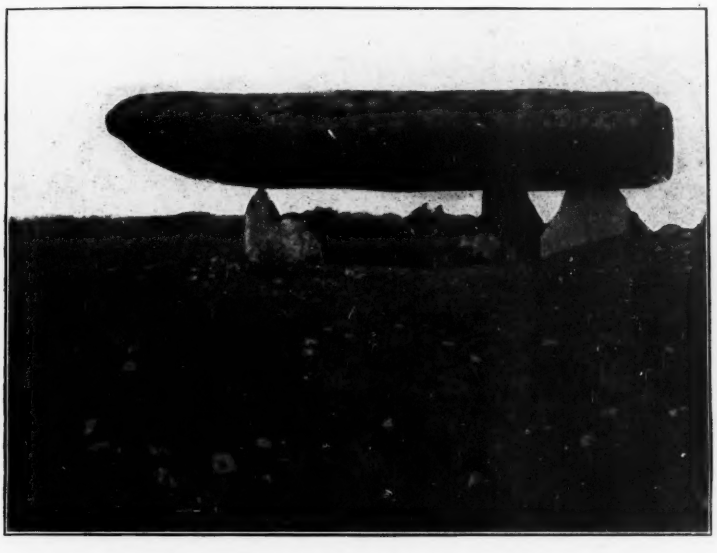

FIG. 6. DOLMEN DES MARCHANDS, LOCMARIAQUER.

the smaller end of an egg and remarkable from the fact that its surface is covered with groups of parallel curved lines, a feature found but rarely in this region. Smaller stones, about six feet high, make up the sides of the chamber, while at the opening of the passage into the chamber are a pair of seven-foot stones, like door posts. The roof is supported on these three larger stones, like an enormous three-legged table. The table top is an immense block of granite, about ten feet wide, fifteen feet long and four feet in thickness. The problem of putting this roof in position is not so difficult as that of the erection of the giant menhir just described. We may imagine the ancient workers filling all around the vertical stones of the dolmen with soil and then sliding or rolling the covering stone into position. But even this calls for an expenditure of an enormous amount of human strength.

Near this "table of the merchants" is another dolmen, "mané rétual," less perfect and less easily studied than its fellow, since it has

not been excavated. Its covering stone, however, is larger, or was before one end was broken off. Judging from the size of a man in a picture which I bought it was forty feet in length, eight in breadth and two or three in thickness.

Some of the other dolmens in the vicinity are nearly as large as these, and some vary by having lateral chambers given off, either from the main chamber or from the passage. None, however, are their equals in the size of the roofing stones, but in most instances the roof is formed of several stones. I recall measuring one roughly as it lay across the dolmen at Locmariaquer—as between eleven and twelve feet in length.

The rock of which these monuments are formed is the common granite of the region. The blocks were probably weathered out from the underlying bed-rock by the elements and needed no quarrying on the part of the unknown engineers.

Speculations as to the time at which these monuments were erected, the people who put them in position and the purposes for which they were intended are numerous in the literature of the subject, some of them as fantastic and absurd as those which ascribe the antiquities of

FIG. 7. DOLMEN OF MANÉ RÉTUAL, LOCMARIAQUER, foreshortened so as to include all of the roofing stones.

Yucatan to the followers of St. Thomas of apostolic times. Usually they are attributed to that mysterious people, the "Druids," whoever they may have been. Certain it is that they long antedated the conquest of Gaul by the Romans, while the relics found in connection with some of them would seem to indicate that they may date back to the second stone age, the neolithic period of the archeologist.

Human interments often occurred at the isolated menhirs and associated with these are found only simple pottery and instruments formed solely of stone and bone. Not a trace of bronze or iron, except where it was clearly of a later and intrusive character. Arrow and spear points, ceremonial stones, etc., closely resembling those of our American Indians, would point far back in the history of western Europe. Yet this is not conclusive, for these objects occur only in connection with human interments and one must make allowance for the well-known conservatism of the priestly class. Among other peoples the objects buried with the dead retained the primitive character long after the race had developed other forms in its daily life. So it may have been here. The fact that these burials were accompanied only by objects of the stone age is not conclusive proof that the people were ignorant of bronze or even of iron.

It is an interesting fact that the passages leading to the chambers of the dolmens are invariably so placed that the openings lie between the points of the rising and the setting of the sun at the summer solstice, possibly indicating that the builders were to a certain extent sun worshippers. From certain considerations of orientation the English astronomer, Lockyer, has figured out the date of the building of Stonehenge as about 1680 B.C., with a limit of probable error of two centuries either way. If his arguments be valid, there is a probability that the monuments of Carnac and Locmariaquer are at least as old.

With such a throwing back of the age of these monuments there is more and more uncertainty as to who built them. The "Druids," who just appear on the pages of written history, were Keltish, but what evidence have we that Kelts dwelt in Brittany or Great Britain a thousand or fifteen hundred years before Christ? We know that other races dwelt in these regions before the immigrant Kelt. Did the Kelt erect these stones or did he find them where they still stand when he came? and did he simply adapt his religious rites to them? Who can say?

We are on a little more certain ground when we come to the purposes of the standing stones, or at least of some of them. As implied above, the isolated menhirs, usually placed on some spot a little above the surrounding country, have, in many cases, been found to stand near some burial, and hence it is probable that they are funeral monuments. Some may also have been boundary stones. The dolmens are also mortuary in character. Apparently every dolmen and allée couverte was formerly buried with earth or rocks, the whole forming a large mound —a tumulus or *galgal*. With the ages the earth in many cases has been removed, either by man or by the elements, leaving the strange "tables" as we see them. In other cases the tumulus still persists and many of these have been explored by modern archeologists, all revealing, in the interior, either a dolmen, an allée couverte, or smaller cairns of stones,

FIG. 8. DOLMEN OF KERVERESSE, LOCMARIAQUER.

each with human bones, frequently mingled with those of the horse and cow.

The way in which one tumulus near Carnac has been explored is interesting. This forms a large mound, over 260 feet long, oval in outline, rising fifty or sixty feet above the surrounding plain, and locally known as Mont St. Michel. On one end of the level summit is a small chapel of St. Michel while on the space in front is an interesting cross of fifteenth century workmanship. As open cuttings would have been expensive (and even impossible in the neighborhood of the chapel), the tumulus has been explored by driving small tunnels through it in every direction. These have later been walled up and roofed in with stone, so that, by the aid of a candle, one may visit all the points of interest in the interior, just as one would explore one of our Indiana or Kentucky caves, seeing all the features found—in this case two dolmens and numerous cairns—as nearly as possible in their original condition.

It would be a tedious task to enumerate, even by name, all of the objects found in these explorations, which were begun in 1862 by the Société Polymathique of Vannes, continued, for the fifteen years ending with his death in 1881, by the Scotchman, James Miln, and since that time by Le Rouzic. The material collected by Miln forms a small but very important museum which he bequeathed to the Commune of Carnac, and which must be visited in order to have a full knowledge of these strange megaliths. Le Rouzic, the present curator, is enthusiastic in his field, gladly welcoming the student, and spending much time in explaining his treasures.

In the first place these plainly show that, whether the orientation

of the dolmens has any significance or not, the tumuli were mortuary in character. Sometimes the body was buried while still in the flesh; in other instances cremation had occurred. At times there were isolated interments; at others the bones are found in large numbers, as if there were collective burials. Along with the human bones occur those of the horse and cow, while burnt clay vases, necklaces of pierced stones and stone implements—celts, arrow and spear points, etc.—accompany the remains. Some of the vessels were apparently new, while others show signs of culinary use. Many of the stone implements have a perfection of surface and edge that would imply that they were never used but were merely votive offerings, ceremonial in character. It is interesting to note that even to this day the peasants of Morbihan prize the arrow and spear points as talismans and call them "*men-garun*"—thunder stones.

But what are the alignments? Here we are in the region of speculation—pure guessing. One may pass by with mere mention the view that they, with the cromlech at the end, are gigantic phallic symbols. Le Rouzic thinks them funereal without being sepulchral in character. He thinks that they might have been connected with the religious rites. The spaces between the rows would afford passages for the faithful assembled for the celebration of the ceremonies, possibly in connection with the collective burials in the tumuli, while the cromlech was the place set apart for the priests.

Whether we can ever arrive at an exact interpretation of these monuments or not, whether we ever know when or by whom they were erected, whether we solve the problems involved in the handling of these immense stones; these thousands of rocks—originally 15,000 or 20,000 in number—scattered over the plains of Morbihan will form one of the most striking of the monuments of antiquity. Possibly we shall get nothing better or more definite, certainly nothing more poetical, than the medieval legend of Saint Cornély which I paraphrase from the version given by Le Rouzic.

Saint Cornély was Pope of Rome, from which place he was driven by the pagan soldiers, who pursued him as he fled before them, accompanied by two cows which bore his baggage and belongings when he was tired. One evening he arrived at the village of Moustoir (two miles north of Carnac). Here he fain would have stopped, but hearing a young girl there abusing her mother, he could not stay. So he went on until he came to a little hill (Mont St. Michel) where he had a view in all directions. In front was was the sea, behind the soldiers in martial array. Further flight was impossible. What could he do? He stretched forth his hand and immediately the soldiers, rank and file as they stood, were changed to stone. Hence it is that one sees the long lines of standing stones to the north of the village of Carnac, and

hence it is that the village church is dedicated to St. Cornély. Pilgrims of all countries flocked to Carnac to invoke the aid of the saint for their ailing beasts, and he, mindful of the aid his cows had given him in his flight, granted their requests. To this day ghosts may be seen at night wandering among the lines of stones, the "soldiers of St. Cornély," while the image of the saint and his cows are carved on the front of the church. The query at once arises, have we here a survival from the old worship of Mithras?

THE ORIGIN OF THE NERVOUS SYSTEM AND ITS APPROPRIATION OF EFFECTORS

II. Receptor-effector Systems

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THE second step in the development of the neuromuscular mechanism is represented by the receptor-effector system, a condition fairly realized in such cœlenterates as the sea-anemones and the jellyfishes and probably recurring in the digestive tubes of the higher metazoans. As an introductory example we may turn to the sea-anemones.

Most sea-anemones (Fig. 1) are cylindrical animals attached to some firm object by their aboral disks and carrying on their oral disks a ring of tentacles surrounding the mouth. This aperture leads inward through a short gullet to a large, somewhat divided, digestive cavity, the gastro-vascular space, which extends throughout the whole interior of the animal even to the tips of its tentacles and is the only cavity within the sea-anemone. The body of the animal is made up of walls of extreme thinness; these walls consist of two layers of cells, an outer one next the sea water, the ectoderm, and an inner one next the gastrovascular space, the entoderm. These two layers are separated by a tough, non-cellular sheet, the supporting lamella.

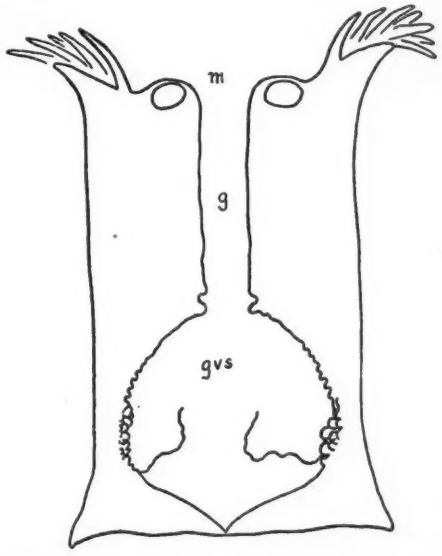

FIG. 1. LONGITUDINAL SECTION OF A SEA-ANEMONE (*Metridium*); *g*, gullet; *gvs*, gastrovascular space; *m*, mouth; *t*, tentacles.

Unlike sponges, sea-anemones are very responsive to changes in their environment. If a fully expanded *Metridium* is disturbed by mechanical agitation, it will quickly retract its oral disk, discharge through its mouth the water contained in its gastrovascular cavity, and

finally cover its tentacles and mouth by puckering in the oral edge of its column. In this contracted state it may remain hours at a time, and when it eventually expands it does so by relaxing its muscles and refilling its body with sea water. A beam of strong sunlight, if thrown upon an expanded *Metridium* several feet under water, will usually call forth the same contraction as mechanical stimulation does.

When the exterior of a *Metridium* is tested locally, its receptiveness for certain stimuli is found to be quite diverse. The animal makes no movements when dissolved food-substances are cautiously discharged upon the external surface of its column, though this very area is sensitive to mechanical stimulation. Precisely the reverse is true of the lips; these organs are easily stimulated by dissolved food-products, but no reaction occurs even when they are punctured by a needle. Both mechanical and chemical stimulation, however, are effective on the tentacles and vigorous responses can be called forth from even distant parts of the body by the application of either of these forms of stimuli to the tentacles. Since these reactions, as just intimated, often involve responses in very different parts of the animal from those to which the stimulus is applied, it follows that we are dealing with a process justly regarded as nervous, for transmission in this case is not accompanied with any observable motion. The surface of a sea-anemone may then be pictured as a true receptor surface partly differentiated in different regions for particular classes of stimuli, but not so far specialized that it can be described as made up of sense organs.

FIG. 2. ECTODERM FROM THE TENTACLE OF A SEA-ANEMONE (*Metridium*); *e*, epithelial layer; *m*, muscular layer; *n*, nervous layer; *s*, supporting lamella.

An examination of the structure of the ectoderm (Fig. 2) will do much to make clear the mechanism by which the reactions of sea-anemones are carried out. The ectoderm of these animals is a modified epithelium in which three definite layers can be distinguished. The outermost of these forms more than half the thickness of the total layer and is a true columnar epithelium. It contains, in addition to ordinary epithelial cells, gland-cells and nettle-cells, and, what is of more importance to us, sense-cells. These sense-cells are long, narrow bodies whose distal ends are armed with a sensory bristle which, under ordinary conditions, projects into the surrounding sea water and whose proximal ends run out into finely branched, nervous processes which intermingle with similar processes from other cells. The complex made by the interweaving of immense numbers of these processes constitutes the second layer of the ectoderm, the nervous layer, and this layer often contains in addition to the large amount of fibrillar material derived

from the sense-cells, numerous multipolar ganglion-cells whose processes add to the fibrillar material already mentioned. A careful study of this fibrillar material has recently been made with the result that a true nervous network has been demonstrated in hydroids (Wolff, 1904; Hadzi, 1909), siphonophores (Schaeppi, 1904) and sea-anemones (Wolff, 1904; Groselj, 1909). In the sea-anemones in particular this network appears to be a perfectly continuous and diffuse one, notwithstanding Havet's previous declaration (1901) to the contrary. The third layer is composed of parallel muscle-fibers that rest against the supporting lamella on one side and are in contact with the nervous network on the other side. The muscle-cells of this layer are much elongated, spindle-shaped cells. These three layers, the epithelial layer, the nervous layer and the muscular layer, constitute the structural elements in the ectodermic neuromuscular mechanism of a sea-anemone.

The nervous type of ectoderm just described covers practically the whole surface of a sea-anemone and has been designated as a diffuse nervous system in contrast to a centralized one. The fact that the nervous layer is more fully developed on the oral disk than elsewhere has given anatomical grounds for the assumption that this portion is a central nervous organ, but, as will be shown later, the physiological evidence in favor of this opinion is so slight that the designation of the nervous system as a diffuse one is more consistent with facts.

From the standpoint of our original analysis, it is quite plain that in the sea-anemones we are dealing with at least two elements of the typical neuromuscular mechanism, namely, receptors as represented by the sense-cells, and effectors as seen in the muscle-fibers. Whether the fibrillar material that intervenes between these two structures represents an adjustor or central apparatus will be discussed after the action of this nervous mechanism has been more fully described.

The feeding habits of the sea-anemones throw considerable light on the physiology of their nervous structures. If particles of meat are dropped on the tentacles of an expanded *Metridium,* they become entangled in the mucus on these organs and are quickly delivered to the mouth, where they are swallowed. If fragments of clean filter-paper soaked in sea water are similarly dropped on the tentacles, they are usually discharged from the edge of the oral disk without having been brought to the mouth. Thus the animal appears to discriminate between what is good for food and what is not. If, however, pieces of filter-paper soaked with meat juice are put on the tentacles, they are usually swallowed as though the sea-anemone had been deceived. On the basis of these simple experiments a still more striking combination can be devised. If a sea-anemone is provided alternately with pieces of meat and pieces of filter-paper soaked in meat juice it will in the beginning swallow in sequence both materials, but after ten or a dozen

trials it will regularly swallow the meat but usually discard the filter-paper. Thus it would appear that the sea-anemone had detected the deception practised on it in the beginning and had learned to circumvent the experimenter. But further observations show how erroneous this interpretation is. If the experiment just described is performed on a limited group of tentacles on one side of the oral disk and, after the animal has arrived at the stage of discriminating between meat and paper, the experiment is repeated on another and distant group of tentacles, it is found that these tentacles and the part of the mouth next them will accept both meat and paper as the first group did and the same process as was used on this group must be repeated on the second group in order to bring it to the stage of discrimination. Thus it is clear that, however we may regard these acts, *Metridium* shows no marked power of making the experienee of one part of its body serve another; in other words, it shows no decided evidence of a central nervous organ.

This conclusion is in substantial accord with the recent results obtained by Fleure and Walton (1907) from experiments on *Actinia* except that they believe that the repeated trials on the tentacles of one side of the circle had in this form a slight influence on those of the other. This influence, however, was so slight that they declared that experience of this kind certainly did not become the possession of the animal as a whole.

Not only is there in these reactions absence of any strong evidence in favor of well-marked central nervous functions in anemones, but it is very doubtful if we are justified in regarding the local reaction just described as a true discrimination. Jennings (1905) has suggested that sea-anemones possess sensations of hunger and that as the experiment proceeds the animal's hunger diminishes and it finally discards when less hungry what it at first accepted. But Allabach (1905) has shown that the same so-called discrimination is arrived at if the sea-anemone is not allowed to swallow anything, but is robbed of meat and paper alike by having these materials picked out of its gullet just as they are about to be swallowed. In fact it seems quite clear that this process of apparent discrimination is in no sense due to centralized nervous functions, but is merely the result of exhaustion. At the beginning of each experiment the receptors are stimulated by the strong juices of the meat and the weaker juice of the paper. As they run down in efficiency, they come to a stage where they no longer react to the weaker stimulus of the paper and respond only to the meat. At this stage apparent discrimination takes place.

Not only do these experiments show no evidence of central nervous functions, but they indicate a decided looseness of nervous articulation. The activity of one side of the body of the sea-anemone has very little,

if any, influence on the other side. Moreover, the fact of intimate local relations between nerve and muscle, as seen in the anatomy of these animals, supports the idea of neuromuscular independence instead of centralized relations. This is well exemplified in the reactions of the tentacles. If a tentacle of *Metridium* is stimulated by food, it turns and twists irregularly and then points toward the mouth. If the same tentacle is cut off and held filled with water so that its original relations in the animal as a whole can be kept in mind, it will be found to react to food as it formerly did, in that it will finally turn toward that side which was originally next the mouth. Hence we may conclude that the tentacle has within itself all that is necessary by way of neuromuscular mechanism for its characteristic reactions and is not dependent for these on such other parts of the sea-anemone as have been regarded as central nervous organs. Physiologically as well as anatomically the sea-anemone seems to possess a diffuse rather than a centralized nervous system, and its neuromuscular mechanism consists of receptors and effectors connected by a nervous net which is composed partly of the nervous processes of the receptor cells and partly of similar processes from ganglion cells.

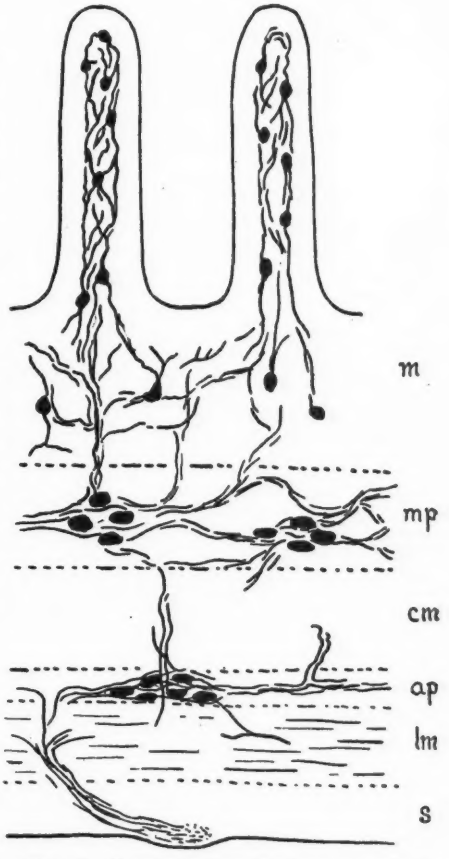

FIG. 3. LONGITUDINAL SECTION OF THE INTESTINAL WALL OF A VERTEBRATE, showing the nervous and muscular constituents; *ap*, Auerbach's plexus; *cm*, circular muscles; *lm*, longitudinal muscles; *m*, mucous layer; *mp*, Meissner's plexus; *s*, serous layer.

The type of neuromuscular mechanism found in the sea-anemones probably also recurs in the digestive tube of vertebrates. This view is supported not only by the action of the intestine, but also by its structure (Fig. 3). Omitting for the moment the outer serous layer and the inner mucous layer of the intestine, both of which have little or nothing directly to do with its neuromuscular mechanism, there are left the outer or longitudinal muscular layer, followed internally by a nervous layer, Auerbach's plexus, which in turn is followed by the cir-

cular muscles on which rests a second nervous layer, Meissner's plexus. Each plexus, so far as is known, is a true nervous net as intimately related to the adjacent muscle fibers as is the case of the sea-anemones. In fact one of the muscle layers and the adjacent plexus in the intestine reproduce very accurately all the essentials of the neuromuscular mechanism of a sea-anemone except the epithelial sense-cells.

Not only is there this anatomical similarity between the neuromuscular mechanisms of the sea-anemone and of the vertebrate intestine, but there is also a physiological similarity as seen in the movements of the digestive tube. The essentials of these movements are well exemplified in the small intestine. In this part of the digestive tube the characteristic movements are segmentation and peristalsis. Segmentation consists in a series of temporary, ring-like constrictions in the intestinal wall that come and go in such a way that the enlarged region of the tube between any two constrictions is the site of the constriction next to appear, and so on. As a result of segmentation, the food is most thoroughly churned and mixed. Peristalsis is a wave-like movement whereby the food is carried posteriorly through the intestine. Usually these two movements go on together in such a way that the peristalsis is combined with segmentation in that the latter becomes somewhat unsymmetrical and cuts each food mass into two unequal parts the larger of which is on the posterior side of the constriction. Hence the food is not only churned but is at the same time moved posteriorly through the intestine.

The small intestine receives nerve-fibers from two extraneous sources, the vagus and the splanchnic nerves, and it might be supposed that these were essential for the movements of the intestine. But as Cannon (1906) has demonstrated, both sets of nerves may be cut, and yet after recovery from the immediate effects of the operation segmentation and peristalsis will be found to go on in the digestive tube in an essentially natural manner. It is thus clear that the vertebrate intestine, like the tentacle of a sea-anemone, contains a complete neuromuscular mechanism within its own wall, and though there is no histological evidence of the presence of receptors reaching from the mucous surfaces of the intestine to the nervous nets within, yet there are sound physiological grounds for assuming the presence of such organs. In that case the type of neuromuscular mechanism in the intestine would be practically identical with that in the sea-anemone.

A second example of a receptor-effector system in cœlenterates is seen in the jellyfishes. In these animals as contrasted with the sea-anemones, locomotion is a well-developed activity, and it is the neuromuscular mechanism concerned with this function that must be considered. The structures involved in locomotion are well exemplified in *Aurelia* (Fig. 4). This common jellyfish possesses on the edge of

its bell eight clusters of sense-organs. Each cluster contains an ocellus, two sensory pits that are probably concerned with the chemical sense, and a sense-club which may be a pressure organ. The sensory portions of all these organs are modified ectoderm and from these portions nerve-fibers pass out as radiating bundles to the ectoderm of the subumbrellar surface. Here they merge into a nervous net which overlies the ectodermic musculature as in the sea-anemones. This musculature forms a circular sheet concentrically disposed with reference to the symmetry of the jellyfish. When the bell of an *Aurelia* is pulsing, the movement is carried out by the more or less general contraction of this circular band of muscle, which is brought back to its original position on relaxation by the elasticity of the gelatinous mass of the bell. The locomotor muscle, then, is a gigantic sphincter that works against an elastic resistance.

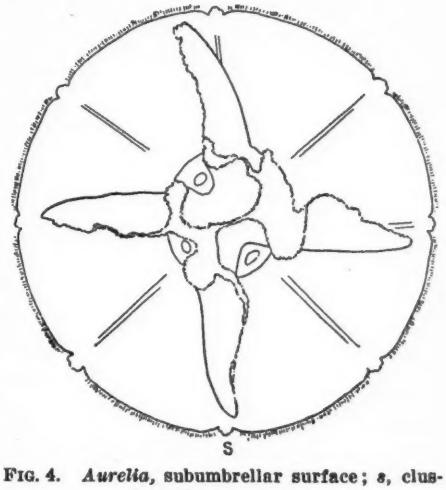

FIG. 4. *Aurelia*, subumbrellar surface; *s*, cluster of sense-organs.

The significance of the various parts of the neuromuscular mechanism in such an animal as *Aurelia* can be determined by experiment. If the eight sense-bodies are removed, the animal will no longer pulse spontaneously, though its muscles may be made to contract by direct stimulation. If all but one sense-body are removed, the bell will pulse with regularity and by artificially stimulating the single remaining body a wave of muscular contraction can be sent over it. It is therefore evident that the sense-bodies act like extremely delicate triggers and thus touch off the contractile mechanism. In this respect, then, the jellyfish is more highly developed than the sea-anemone, for the latter possesses no such specialized and delicate receptors.

The wave of contraction that passes over a bell when one of its sense-bodies is stimulated, may be either a purely muscular phenomenon or may be the result of nervous transmission through the nervous net whereby one region after another of the musculature is brought into action. The fact that this wave is not checked when the bell is cut even in a most irregular way provided the subumbrellar epithelium is still continuous, favors the nervous rather than the muscular interpretation. But stronger evidence on the nervous side than this has come from an entirely different direction. Mayer (1906) has shown that the subumbrellar epithelium of *Cassiopea* after removal will readily regen-

erate, and that in regeneration the nervous net forms earlier than the muscles. By taking jellyfishes at the appropriate stage in regeneration, it was found that a stimulus applied to one side of a regenerated area was followed by a muscular response on the other side of this area without any observable movement in the area itself. Hence transmission through the regenerated region must have been by nervous means, doubtless by the nervous net.

In jellyfishes the nervous net will transmit apparently in any direction and in this respect it is in strong contrast with the central nervous organs of the higher metazoans, where, especially in the vertebrates, a polarized condition generally prevails. Thus in the spinal nerves of vertebrates, it is easy to send impulses through from a dorsal root to a ventral one, but impossible to send them in the reverse direction. Apparently the cord contains some structure on its path of conduction that is valve-like and allows impulses to pass in one direction only. Such a condition does not exist in the nervous net of the jellyfishes.

FIG. 5. NEUROMUSCULAR CELL (black) in place in a columnar epithelium.

The neuromuscular organs of the cœlenterates have been considered by so many investigators as the most primitive in the animal kingdom that it is not inappropriate to consider at this place the relations of some of the older views on this subject to those expressed in these articles.

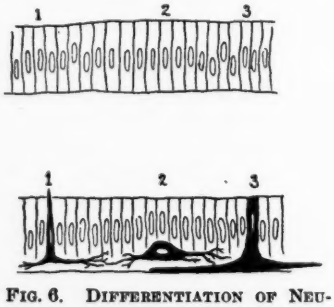

FIG. 6. DIFFERENTIATION OF NEUROMUSCULAR CONSTITUENTS FROM AN INDIFFERENT EPITHELIUM. The upper figure represents an indifferent condition containing three cells which subsequently (lower figure) differentiate into a sense-cell (1), a ganglion-cell (2), and an epithelial muscle-cell (3).

The discovery by Kleinenberg (1872) of the so-called neuromuscular cells (Fig. 5) in *Hydra* led this investigator to the belief that these cells represented a complete neuromuscular apparatus in that each cell-body could be regarded as a receptor and its fibrous portion as an effector. By growth and cell division, according to Kleinenberg, separate receptors and effectors would be differentiated simultaneously from such single cells.

The simultaneous differentiation of nervous and muscular elements (Fig. 6) was also accepted by the brothers Hertwig (1878), but in their opinion the two types of tissue did not arise from a common cell as claimed by Kleinenberg, but from separate cells which became simultaneously differentiated, some to form nerve-cells (sense- and ganglion-cells) and others to form muscle-cells. This view has come to be commonly accepted by the majority of investigators.

The independent origin of the nervous system and its secondary

connection with the musculature has been advocated by Claus (1878) and by Chun (1880), but a nervous system without effectors is, as Samassa (1892) and Schaeppi (1904) declare, scarcely conceivable.

The opinion about the origin of nervous and muscular tissues as expressed in these articles is opposed to the various theories stated in the preceding paragraphs in that muscular tissue is regarded as the ancestral tissue and nervous tissue is supposed to have formed secondarily and as a means of bringing muscular tissue into action with greater certainty than direct stimulation would do. According to this view the primitive state of the neuromuscular mechanism is to be seen in such animals as sponges, which possess muscles but no true nervous organs; and the neuromuscular or, better, epithelial-muscular cells of the cœlenterates represent these primitive effectors to which have been added a diffuse system of receptors as seen in the sea-anemones or a specialized system as in the jellyfishes. In both instances the receptors and effectors are related through a nervous net.

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THE VARIATIONAL FACTOR IN HANDWRITING

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HANDWRITING, bearing as it does the cachet of individuality, has always interested those to whom things human make their intimate appeal. Curious observations relative to it have long been current, the existence, for instance, of national as well as family and personal chirographies; the perversions of it that take form as mirror-writing or even—it is said—as inverted writing; the whimsy shown by the bizarre characters, by the tendency to irrelevant and extravagant flourishes in the writing of those suffering from certain forms of mental disorder. Attention has been called to the similarity existing between a man's handwriting and the manner in which he walks or gesticulates. It has been claimed that age and sex and profession leave their impress upon writing, that the pencraft of the painter mirrors minutely the grace and distinction that marks the sweep of his brush across the canvas. Carried out boldly such speculations venture even the claim that the handwriting of any individual would be found to resemble the characteristic tracings shown by his pulse and respiration and fatigue curves. Nor is the interest in the variational aspect of handwriting restricted to recording the diversities in penmanship from individual to individual; it is also engaged in noting variations from day to day in the handwriting of any given person under the influence of fatigue or emotion or disease. But, however numerous, such observations and however legitimate the speculations they engender, it remains for the physiologist and the psychologist, with the aid perhaps of the sociologist, to compass the scientific study of the variational factor in handwriting.

The ground, however, has been broken. As has frequently been the case in the history of research, the claims of a pseudo-science, at once provocative and suggestive, have stimulated inquiry. In this case, graphology, the art that would find in handwriting revelations of intelligence and character, has been the direct cause of a series of investigations. On the other hand, modern psychological theory with its increasing emphasis upon behavior, upon the motor aspects of life, could not long ignore the opportunity for study presented by this most complicated and subtle act of individual expression.

Two lines of investigation have accordingly been inaugurated by the

psychologist; the one interested in the functional significance of the act of writing as the expression of individuality; the other interested in a minute analysis of this motor series, seeking to determine the laws of expression that govern this particular act. In both investigations methods of research are being worked out with the ingenuity so characteristic of scientists of to-day. Each investigation as it progresses will be found to encroach upon the other. From the two will come the future science of handwriting. A résumé of the work that has already been done has perhaps its value at the present time.

First of all it may be profitable to consider the investigations that have sought to determine under scientific control whether or not the graphologists have made good their claims. It is to France that we owe, not only the most carefully wrought-out system of graphology, but also the most carefully thought-out control of that art. In an investigation covering many months, Alfred Binet, the director of the psychological laboratory at the Sorbonne, planned and executed a series of carefully controlled experiments designed to test the ability of the graphologists to determine from handwriting the sex, the age, the intelligence and the character of the writer. Binet, who guarded carefully against all sources of error, so planned his experiments as to be able to state in figures the percentage of error in the interpretations of the graphologists and thus render possible a comparison of the graphologists' successes with those that might reasonably be expected if chance alone determined the outcome. The results showed unmistakably that the graphologist was able to determine with but a small percentage of error the sex of the writer and also, but with less certainty, the intelligence of the writer. The interpretation of age and character offered still greater difficulties. To render the tests perfectly definite and to avoid the error that might arise from the personal equation in estimation of intelligence and character, Binet in his tests upon them made use, on the one hand, of the handwriting of men famous in literature and science and, on the other hand, of specimens of the handwriting of great criminals, whose biographies were matter of legal record.

Binet's investigation, apart from his general conclusions, brought out some interesting facts. He found, for instance, that there existed not only very great differences in the skill with which different graphologists made their interpretations, but also that there were those uninitiated in the art whose readings at times even the professional graphologist might envy. An observation akin, in a way, to the common experience that some people remember and recognize handwritings, as others do faces, with extraordinary facility and accuracy. Minute differences have for them undoubtedly a value not experienced by others. Binet found, moreover, that the professional's skill in

diagnosis far outran his ability to ground his judgment on definite graphic signs. His reading was the translation into words of a general impression, somewhat similar, we may assume, to that received by the skilled reader of the human countenance. Moreover, in at least one instance, and that in the case of a non-professional, the judgments, based on intuitions, that is, non-reasoned-out impressions, were achieved in a state of passivity that we are familiar with as characteristic of automatic activities of different sorts.

Accepting these results, the investigation is obviously only well initiated, for one is next anxious to press home the question that asks the cause of such differences. It is not enough, for instance, to know that Binet's graphologists were able under highly favorable conditions to distinguish in ninety per cent. of the tests the sex of the writer; it is not enough to know that, to a certain extent, they were able to base their judgments upon the presence or absence of certain graphic signs; one would also know in detail what determines each sign of sex, whether at the last they are due, as Binet himself asks, to profound physiological or psychological causes, or, rather, are the outcome of the social environment so different in the case of the two sexes.

We are here brought face to face with the old question that has confronted all investigators of sex-differences. It is evident, however, that the question of the social environment is, in this instance, a controlling one not merely in the discussion of the revelation of sex in handwriting, but also in that of the revelation of intelligence; for there exists a peculiar environment for talent as well as for sex. Indeed, it appears that the investigation of handwriting must be socio-psychological in nature. Unconscious imitation, social suggestibility doubtless play an important, if not all-important, part in determining writing characteristics. On the whole, therefore, it is not surprising that the experts were more successful in distinguishing marked differences in intelligence than in determining the nature of the individual superiority. They perceived the class characteristic, as it were.

The overlapping of the writer's environments, social and professional, must further complicate the matter. The cases cited by Binet of writing that gave evidence of reversion of signs: the writing, for instance, of a young woman scientist that the graphologists unanimously judged to emanate from a man, or the handwriting of a man like Renan that the graphologists marked as coming from a man of inferior mental ability are of particular interest in this connection. Such cases would probably repay a detailed investigation not only of the psychology of the individual, but also of his environmental history.

It is, perhaps, because character, within certain limits, does not produce segregation of classes that the experts showed little accuracy in their judgments of moral qualities from handwriting. Their failure,

for instance, to find in the handwriting of a young woman murderer, who was of some social position, evidence of more than feminine instability and coquetry is instructive; for the case was an aggravated one of the murder, by poisoning, of three innocent victims—husband, grandmother and brother—for the sake of trifling gain.

A further control of these experiments, an attempt to diminish the masking effect of class-imitativeness, might be achieved by international work, by tests involving the discovery of similar graphic signs in the writing of individuals separated by race and training. A repetition of Binet's test as to the possibility of distinguishing sex-differences might be of value in this country where sex-segregation in education is much less pronounced than it is in France.

Other sociological aspects of handwriting might no doubt be investigated. The variation in individual chirography due to the nature of the letter written, be it of social import or a business note; the change in penmanship that comes with the change of the relation of the writer to the one addressed—all such observations, vague as they are at present, merit consideration. Most suggestive of all is the shift in style that comes when the writer addresses his own eye alone, yielding himself to the fervor of composition or the mental dissipation of being "off parade." But observations under such conditions must at best be made stealthily. A hint at the possibility of the intrusion of one's mental privacy and, conscience or vanity on the alert again, one's writing hastens to resume its conventional legibility.

The revelations of the autograph as a mental photograph, a graphic representation of social relationships, have never been fully appreciated by the sociologist, although the world at large has always accepted a famous man's autograph as secondary in interest to his photograph alone. The pretense, the dignity, the reserve, the finesse with which one faces the world finds copy in the ostentation, the simplicity, or the ambiguity with which one signs one's name. Indifferent though one may be in penmanship in general, there is something intimate and personal in the autograph that arrests one's interest, so that in the somewhat fantastic world of images, of symbols, it often happens that one adopts a mental picture of his own autograph as the official representative of himself in the counsels of thought.

In any case it is evident that there is a psychology as well as a sociology of handwriting. Tremendously complicated as the problem of diagnosis of individual traits from those tiny strokes of the pen appears, it is yet a legitimate problem of science; for the more progress psychology makes, the more evident it becomes that there is not a mode of expression which is not rooted to its finest detail in the complex psycho-physical organism. Meanwhile, it is fortunate that the task of identifying graphic signs should not be left wholly to the

intuitions of the graphologist. Experimental work that seeks to induce variation in writing through a control of outer conditions must in time correlate certain definite variations in conditions with variation in such aspects of writing as size, speed, accuracy in alignment, inequality of control and the like.

The experimental investigations, spoken of above, have attacked the problem at this point. Abandoning any attempt to deal with the more complicated aspects of chirography as an expression of individuality, they have confined themselves to an accurate analysis of such factors as speed of movement and its variations; the length and significance of writing-pauses; measurement of pressure and its variations; comparison of the accuracy of control for right and left hand; elimination of visual control; minute analysis of finger, wrist and arm movements involved in handwriting, with an assignment to each of its rôle. Such an investigation, so far as it confines itself to mere analysis, is obviously but a part of the general investigation of voluntary action. But the discovery of methods of accurately registering minute variations in writing speed, pressure, amplitude and musculature is necessarily preliminary to an accurate determination of the correlation between particular psychic traits and their expression graphically.

An illustration of what may be expected from the perfecting of the technique of registration of speed, pressure and amplitude of writing is to be found in the report of a piece of work carried out some years ago in a German laboratory, where it was discovered that increased difficulty in mental work showed itself in written expression by increased pressure or by decrease in the size of the written characters. The former way of meeting the difficulty seemed to be characteristic of men; the latter, characteristic of women.

Variation in the amplitude of written characters involves doubtless many important considerations relative to the facilitation and inhibition of movement. Writing with attention preoccupied or distracted results variously in the enlargement or dwarfing of characters, an alternative result that seems to depend upon deep-seated tendencies of the individual. If, as facts apparently show, the individual who is the more automatic in his activities responds to distraction with an increase in the size of characters used; while one less automatic, one whose attention—though sometimes in a maimed condition—is always at the helm, gives evidence of the mental difficulty by a decrease in amplitude, a decrease that bears witness to the inhibition at work, then a very simple test is at hand by means of which individuals may be grouped under the two types that have been labeled, somewhat ambiguously, motor and sensory. If it should be shown further that this difference cuts through all the mental activities of the human being, progress would have been made in the difficult matter of the classification of mental types.

Whatever more extended observations may show, the writer of this paper has found it a very simple matter to pick out individuals who will make good subjects for muscle-reading—an experiment that succeeds best with those whose movements are most automatic—by a preliminary test in which the subject, blindfolded, is required to write his name rapidly in sequence while counting aloud by a given interval, say by 13's. The writing of those individuals who would serve best in the proposed test shows a progressive enlargement and, moreover, characteristic pen-lapses.

The question may then be raised whether such difference in mental type reveals itself in normal handwriting, and an affirmative answer seems not presumptuous, although a detailed study of handwriting from this standpoint has not, so far as the writer knows, been instituted experimentally. It should be noted, however, that in the thought process which accompanies writing during composition, momentary distractions occur frequently, for thought, even in the case of rapid penmen, is apt to run ahead of the writing. Who does not number among his correspondents those whose final letters trail off into an indistinguishable scrawl; and others who end with a flourish that marks well the motor abandon? Characteristic revelations, no doubt, although interpretation as yet must be exceedingly diffident.

It is interesting to note in this connection the interpretation graphologists put upon the size of writing as indicative of individual traits. Distinction, power, frankness, honesty are held to reveal themselves by magnified writing either throughout writing as a whole or at the termination of words. Minute writing throughout or at the close of words is held to indicate, in the case of superior intelligence, artifice or preoccupation with metaphysical or other minutiæ; in the case of inferior minds, miserliness. Usually, the graphologists emphasize legibility of terminal letters as highly indicative of frankness; while, on the other hand, the tendency to terminate letters in filiform fashion as evidence of a veiling of self. Mere exhibition of documents from persons of known characteristics seems, it must be said, inadequate proof of such propositions. Variations from the normal in the handwriting of any individual would under defined conditions be of more value for general interpretative purposes than would variation from one person to another. Nor can facile analogies appear worthy of serious attention until the causal relation between certain temperamental traits and the facilitation or inhibition of movement is better understood.

The attempt to study handwriting in the light of psychological analyses already in progress bids fair to help analysis, as well as to increase our knowledge of the psychology of handwriting. The relation of the inner word to the outer visible one has long interested

psychologists and pathologists, particularly in connection with the investigation of agraphia, that is, loss or impairment of the power to write. But the interest in such difficulties has centered largely in the fact that study of them might contribute to the physiological problem of the localization of cerebral function. If more than this, the interest has usually limited itself to an analysis of the situation in sensory terms; the details of the resulting expression have been but little studied. The growing interest in the psychology of lapses, both linguistic and graphic, and the development of a technique for such study is of great promise. In the case of the graphic lapse there is need not merely of the tabulation of what kind of errors are made, but also a reproduction of the writing in which the errors are found. Will such writing show characteristic variations in amplitude, pressure, and the like?

Even apart from the question as to the effect of a "hitch" in the process upon the appearance of writing, we may ask whether the general appearance and characteristics of writing are affected by the type of thought-process normal for any given individual. An intensive study of imagery types has always recognized, although with considerable divergence of opinion as to details, the varieties of the word-image. The word mentally seen or heard, spoken or written, has been found to play an important part in the complex thought-processes that underlie the consciousness of meaning and the possibility of its expression. The question of significance here is how far one sort of verbal imagery is potent in initiating the written word of any individual and whether any difference in written gesture marks off the individual who habitually indulges himself in visual imagery from the man who is more motor in type or more dependent upon auditory images.

Back even of this question lies the more deep-cutting one of the significance to the whole mental life of the predominance of the sensations, perceptions and images of a ruling sense. At what point and to what extent in the process of learning to write does the visual-motor coordination fall under the ruling sense and what effect has such subordination upon the general appearance and character of the resulting chirography? One feels certain that the handwriting of the man visually inclined must differ from that of the man preoccupied with motor details, but is unable to specify the difference. Yet the problem does not remain insoluble, for there are simple methods of determining the part played by each sense in control of the writing of any individual. Accompanying sensations such as those of sight and sound may be eliminated from the situation and the effect noted; or conflict with visual or auditory or motor images may be introduced and the results recorded. The investigation of the varieties of writing-control with the relation of each to writing-appearance offers a tempting field for work. But here speculation must wait upon the facts.

In such an investigation, however, that peculiar perversion of writing known as mirror-writing, because legible only when seen in a mirror or in transparency, may be utilized experimentally. The fact, on the one hand, that many left-handed children write in such a fashion from the first; and, on the other hand, that right-side paralytics, forced to the use of the left hand often resort spontaneously to such a form in their written communications has brought it about that the investigation of mirror-writing has been in the past largely turned over to those whose interests were either pedagogical or pathological, and has led to the conclusion that mirror-writing is either the normal writing for the left hand of all individuals or the normal writing for the left hand of the left-handed only. An examination of the evidence cited in support of these propositions or a discussion of the explanations that are used to ground them is not now in place. What is of interest is the insistence upon the need of further experimental work on induced mirror-writing with the hope of getting more light upon the relation here involved between the written characters and their visual significance. For the first question that arises is this: How can mirror-writing prove visually satisfactory, however "motorly" comfortable? And the answer to this question involves the whole problem of the relation of visual and motor control. It is probable that artificially induced mirror-writing is a simple device for determining to what extent the divorce between motor and visual control has resulted in the case of any one individual.

But the relation of handwriting to emotional temperament, as well as its relation to imagery types, merits consideration. Variation in expression under emotional disturbance has long been a special subject of experiment. Little attempt, however, has been made to compare the results so obtained with the appearance of writing under emotional tension. To be sure, the graphologists cite a tendency to elevate progressively the line of writing as an evidence of mental exaltation, of joy or ambition, while a fall in the alignment is indicative of the depressive emotions, self-distrust, sadness, melancholy. Again, a strongly marked tendency toward centrifugal or centripetal movements is held to indicate, on the one hand, ardor, simplicity, activity, uprightness, and, on the other hand, slowness, lack of spontaneity, egoism. These observations, if confirmed, need to be brought into definite correlation with the results obtained in experimental work; and in this connection the graphologists do appeal to the experimental interpretation of movements of expansion and of flexion.

Again, the observation seems in point that variations from the normal in the handwriting of any individual are, under defined conditions, of more value for general interpretative purposes than is variation from one individual to another. Some attempts to induce arti-

ficially, by means of hypnotic suggestion or provisional deceit, changes in the mood or even in the personality of a given reagent have indeed been tried in France. The results, although striking and interesting are somewhat general in nature nor is the method beyond criticism.

The dependence of style of writing upon suggestion has already been spoken of in emphasizing the rôle social suggestion plays in determining writing-types. Experimental work may investigate the influence of this factor. An incident in which a friend of the present writer, in signing the latter's name to a lecture-ticket, unconsciously imitated the writer's signature shows how extensively suggestion may operate. Reports of the character of writing during hypnosis offer material for study. Detailed reports as to the characteristic appearance of such writing are, however, wanting.

Professor Janet, of the Collège de France, urges, and with reason, that experimental graphology should begin with studies in pathological graphology, studies on the effect upon handwriting of diseases of motility and sensibility, or of specific diseases, such as those of respiration and of circulation. From the more pronounced modifications of handwriting transitions may then be made to its more delicate inflections.

This recourse to pathology bids fair to prove increasingly fruitful. Physicians have long been aware of profound modifications of handwriting through disease and have utilized such modifications in diagnosis. Considerable material has been collected and published by them in connection with their discussions of insanity, hysteria, epilepsy, paralysis and the like. Their interest has been, however, often practical rather than theoretical, and it is only with the increasing interest in the specific problem of handwriting that the full value of their documents becomes evident. Moreover, the failure to record in a particular instance specimens of the normal as well as of the perverted writing is often regrettable. Experimental work upon pathological writing has, however, already been resorted to in the attempt to determine the changes in writing induced by the use of alcohol and various other drugs.

A highly interesting case of pathological writing is that known as automatic writing, writing of which the writer is either not conscious at all or else conscious only of the movement and its result without feeling in any way responsible for the act. In connection with such automatic writing one would like to have not only an analysis of the mental state, but also detailed information of the variation from the normal in terms of speed, amplitude, alignment and pressure of writing. It is worth noting that Professor Janet has published examples of mediumistic writing, and that Dr. Prince, in his recent book on "The Dissociation of Personality," has reproduced the handwriting of a secondary personality.

Much work to-day still needs to be done in the collection, according to well-formulated plans, of material for the study of handwriting. In the matter of family resemblances in chirography, for instance, there is scarcely any material at hand, a fact not surprising since such work of collection must needs run over years. An instructive series of family autographs would be one showing handwriting at different periods of development. Any resemblance here in the handwriting at the same period of life of individuals differing considerably in age would testify directly to hereditary motor tendencies of some fineness, since suggestibility as a contributing cause would be ruled out.

Doubtless the day is far in the future when we shall be able to solve such historic enigmas as Mary, Queen of Scots, by an appeal as Tarde, the French sociologist, suggests, to her handwriting; or be proficient enough in the art of interpretation to proffer our services, as other enthusiasts predict, to the benevolent advocates of scientific match-making; but such suggestions carry with them a faith in the interpretation of this finest, subtlest of movements which time will perhaps justify. Nor will a scientific interpretation of individual chirography come merely to gratify an idle curiosity or a secret malice. It will be of immense value. All the arts remedial and educative will have need of it. Physician and educator, criminologist and sociologist, will make their appeal to it. Strange, if in time these tiny written gestures should be found to be all-revealing; if in them should be found the most intimate expression of the dramatic instinct.

JANE LATHROP STANFORD

A Eulogy[1]

By President DAVID STARR JORDAN
STANFORD UNIVERSITY

I AM to tell you to-day the story of a noble life, of one of the bravest, wisest, most patient, most courageous and most devout of all the women who have ever lived. I want to give to those of the university to whom its founders are but a memory some lasting picture of the woman who saved the university, which she and her honored husband founded in faith and hope, and who thus made possible the education you are receiving. I want to make my story as impersonal as I can, as though I spoke not for myself but for all of you, men and women of Stanford, with all gratitude towards the many who have helped in the great work, and with all charity towards those whose interests or whose conscientious convictions ranged them on the other side. If I am successful, you will see more clearly than ever before the lone, sad figure of the mother of the university, strong in her trust in God and in her loyalty to her husband's purposes, happy only in the belief that in carrying out her husband's plans for training the youth of California in virtue and usefulness she was acting the part to which she was assigned.

We have often said that Stanford University belongs to the Stanford students. It was the free gift of the founders, man and woman that were, to the students, the men and women that were to be. It is your university, yours and yours only, as once it was theirs.

But we must not interpret this gift too narrowly. It is not yours, you students of to-day, to have or to hold in any exclusive way. The university belongs to all the students, those who have been here, some ten thousand in all, those who are here to-day, seventeen hundred more or less, and those who are to come. Before these we count as nothing, for the students to come will number for each century about a hundred thousand. And there are many of these centuries, for the world is still very young, and a university once firmly rooted is as nearly eternal as human civilization itself can be. The university stands for the highest thought and wisest action possible for man, and the need of a university must endure so long as man exists; and that will be for a very long time. Man is bounded by the limits of space, but the race once established on this planet of ours, we see no limit of time, no prospect of a

[1] Founder's Day address at Stanford University, March 9, 1909.

twilight of gods in which the darkness shall fall on the world because universities are no longer needed. The center of gravity of Stanford University, of its student body, and of its influence on civilization, is hundreds of years, thousands of years ahead.

To the students of to-day, the professors of to-day, and the trustees of to-day, the university to-day belongs, but not as a personal possession; only as a sacred trust. It is our first duty to see that its good name and its good work are kept untarnished and unimpaired. It is for the students to see that no custom of idleness or of dissipation, no fashion of cynicism or of disloyalty ever becomes hardened into a tradition at Stanford University. It is for the professors to strengthen them in this decision, and to point out the best that men have ever thought or done, to lead the way to gentle breeding and the enthusiasm of noble thought. Now, as ever, "the university must welcome every ray of varied genius to its hospitable halls," that their combined influences may "set the heart of the youth in flame." It is for the Board of Trustees and for the university executive to act as the balance wheel, guarding jealously the funds of the institution, that the generous present may not starve the future, and to see that no neglect or perversity of student or teacher shall work any permanent harm to the university whole. For the university must ever be infinitely greater than the sum of all its parts. For its largest part is never present for our measurement, and this part we can not measure is the sum of all its future influence.

This university was founded on love in a sense which is true of no other. Its corner-stone was love—love of a boy extended to the love of the children of humanity. It was continued through love—the love of a noble woman for her husband; the faith of both in love's ideals—and as an embodiment of the power of love Stanford University stands to-day.

It is fitting that these statements should not stand as mere words. I wish that in your hearts they may become realities. Not many of you as students have seen Mrs. Stanford. The last of the freshmen classes which she knew shall graduate as seniors a few weeks hence. None of you have known Leland Stanford, broad-minded, stout-hearted, shrewd, kindly, and full of hope, a man of action ripened into a philosopher. Our university has now reached its eighteenth year. During the first two years of its history, it was the hopeful experiment of Leland Stanford. The next six years its story was that of the heart throbs of Jane Lathrop Stanford, and the ten years following, with all their vicissitudes, have been years of calmness and certainty, for the final outcome is no longer open to question.

It is my purpose this evening to tell a little of the story of the six dark years, the years from eighteen ninety-three to eighteen ninety-nine, those days in which the future of a university hung by a single

thread, but that thread the greatest thing in the world, the love of a good woman. If for an instant in all these years this good woman had wavered in her purposes, if for a moment she had yielded to fear or even to the pressure of worldly wisdom, you and I would not have been here to-day. The strain, the agony, was all hers, and hers the final victory. And so any account of these years must take the form of eulogy. Eulogy, in its old Greek meaning is speaking well, and my every word to-day must be a word of praise. It is proper, too, that I should speak these words, and even that I should give this history from my own standpoint, because there were few besides myself who knew the facts in those days. Most of these facts even it is well for all of us to forget. For the rest, the facts in issue will appear only as needed for the background, before which we may see the figure of Mrs. Stanford.

I first saw the Governor and Mrs. Stanford at Bloomington, Indiana, in March, 1891. At that time, Governor Stanford, under the advice of Andrew D. White, the President of Cornell, asked me to come to California to take charge of the new institution which he was soon to open. He told me the story of their son, of their buried hopes, of their days and nights of sorrow, and of how he had once awakened from a troubled night with these words on his lips: "The children of California shall be my children." He told me the extent of his property and of his purposes in its use. He hoped to build a university of the highest order, one which should give the best of teaching in all its departments, one which should be the center of invention and research, giving to each student the secret of success in life. No cost was to be spared, no pains to be avoided, in bringing this university to the highest possible effectiveness.

In all this Mrs. Stanford was most deeply interested, supporting his purposes, guarding his strength, alert at every point, and always in the fullest sympathy.

Mr. Stanford explained that thus far only buildings and land had been given, but that practically the whole of the common estate would go in time to the university, when the founders had passed away. If he should himself survive, the gift would be his and hers jointly, though the final giving would be left to him. If the wife should survive, the property would be hers, and in her hands would lie the final joy of giving. Mr. Stanford gave his reason for not turning over the property at once, for this might leave his wife no controlling part in the future. It was not his wish that she should sit idly by while others should create the university. So long as she lived, it was his wish that the building of the university should be her work.

This attitude of chivalry in all this needs this word of explanation, for it shaped the whole future history of the university endowment. It was the source of some of the embarrassments which followed, and perhaps as well of the final success.

The university was opened on the first day of October, 1891, a clear, bright, golden, California day, typical of California October, and full of good omen, as all days in California are likely to be. There were on the opening day 465 students, with only 15 instructors, and the first duty of the president was to telegraph for more teachers, laying tribute on many institutions in the east and in the west.

Two years followed, with their varied adventures, which I need not relate to-day. It was on the twenty-second of June, 1893, that the university community was startled by the sudden death of Leland Stanford.

It is not my purpose now to praise the founder of the university. One single incident at his funeral is firmly fixed in my memory. The clergyman, Horatio Stebbins, in his stately fashion told a story of the Greeks doing honor to a dead hero; then, turning to the pall-bearers, stalwart railway men, he said: "Gentle up your strength a little, for 'tis a man ye bear." A man, in all high senses, in that noblest of words, a man! was Leland Stanford.

After the founder's death, the estate fell into the hands of the courts. The will was in probate, the debts of the estate had to be paid, the various ramifications of business had to be disentangled, and meanwhile came on the fierce panic of 1893. All university matters stopped for the summer. Salaries could not be paid until it was found out by the courts by whom and to whom salaries were due. All incomes from business ceased. There was no such thing as income visible to any one, least of all to the great corporations.

After Governor Stanford's death, Mrs. Stanford kept to her rooms for a week or two. She had much to plan and much to consider. From every point of view of worldly wisdom, it was best to close the university until the estate was settled and in her hands, its debts paid and the panic over. Her own fortune was in the estate itself. Outside of her jewels, she had practically nothing of her own save the community estate, and this could not be hers until the payment of all debts and legacies had been completed. These debts and legacies amounted as a whole to eight millions of dollars. In normal times, there was hardly money enough in California to pay this amount; but these were not normal times, and there was no money in California to pay anything.

After these two weeks, Mrs. Stanford called me to her house to say that the die was cast. She was going ahead with the university. She would let us have whatever money she could get. We must come down to bed rock on expenses, but with the help of the Lord and the memory of her husband, the university would go ahead and fulfil its mission.

It was no easy task to do this, as one incident will show. There could be no regularity in the payment of salaries. In the eyes of the law the university professors were Mrs. Stanford's personal servants.

As such, it was finally arranged that they receive a special allowance from the estate. This allowance as household servants paid their salaries, and a registration tax of twenty dollars per year on each student had to cover all other expenses. But these two sources of income did not come at once, and the great farms run as experiment stations were centers of loss and not of income.

A single incident will make this condition vivid.

At one time in August, 1893, Mrs. Stanford received from Judge Coffey's court the sum of $500 to be paid to her household servants. It was paid in a bag of twenty-five twenty dollar gold pieces. Mrs. Stanford called me in and said her household servants could wait; there might be some professors in need, and I might divide the money among them. I put the money under my pillow, and did not sleep that night. Money was no common thing with us then. Next morning, on Sunday, I set out to give ten professors fifty dollars apiece. I found not one who could give change for a twenty dollar gold piece, and so I made it forty dollars and sixty dollars.

The same afternoon after I had gone the rounds $13,000 was brought down from the city for us other household servants. This sum was distributed, and then Mrs. Stanford sent word that as we had some money now perhaps we could spare her the $500. I drew a check for the sum against a long-vanished bank account, and covered the amount in the morning with the aid of some of my associates.

This incident again will explain why for six years the professors were paid by personal checks of the president, and why these were not always issued regularly, nor for the full amounts. We were all struggling together to be able to issue them at all. There was no certainty ahead of us. Most of the property was of such a character that it could not be divided, but must go in blocks of millions, if it went at all, and no one with millions at his disposal seemed inclined to invest it anywhere. The estate held a one fourth interest in the Southern Pacific System, and of all its many ramifications. Kept together, it could maintain itself, but if any division were made the smaller part might be subject to the process known as "freezing out."

I pass by many minor incidents of struggle and economy. The farms had to be abruptly closed, and then to be made to yield an income. This required wise management and rigid economy at the same time, but for all this Mrs. Stanford proved adequate. She learned her lessons as she went along, and came to take a wholesome pleasure in the Spartan simplicity of her life. If all else failed, there were the jewels to fall back upon; and she steadily refused to consider the advice (almost unanimous) of her counsel to close the university or most of its departments until some more favorable time. In 1895 she invited the pioneer class, then graduating, to a reception in her city home, one reason being that it was the last class that could ever gradu-

ate. We had nothing to run on, save the precarious servant allowance, then fixed at $12,500 per month, and liable to be cut to nothing at any day. Our expenses for 1893 had been nearly $18,000 per month. Sometimes we could sell a few horses from the stock farm, but it was never clear that the stock farm belonged to the university and not to the Stanford estate, and every dollar we gained this way piled up the possibilities of litigation. All these days were brightened by the steady support of her friends and advisers, Samuel F. Leib, Timothy Hopkins and Russell Wilson. Mr. Hopkins furnished the Library of Biology and paid unasked many minor expenses, his left hand not taking receipts for what his right hand was doing. No one can tell how much the university owes to these men, who in the darkest days planned to make the future possible. Very much too the university owed to the fraternal devotion of Mrs. Stanford's brother, Mr. Charles G. Lathrop, who cared for with sympathetic hand the scanty receipts and scanty fragments of these harassed days. The warm sympathy of Thomas Welton Stanford came from across the seas. His gift of the Library Building came as a shadow of a great rock in a weary land.

At last, adjustment of one kind after another being made, there was a glimpse of daylight, when we were thrust without warning into still darker night.

The government suit for fifteen millions was brought for the purpose of tying up everything in the Stanford estate until the debts of the Central Pacific Railway were paid. It was not claimed that the university owed anything, or that the Stanford estate owed anything, or that the railway owed anything, on which payment was due, and as a matter of fact the Southern Pacific Company paid in full every dollar it owed to the government as soon as it became due, and with full interest. There was never any reason to suppose that it would not do so, and never any reason to suppose that it could not afford to pay this debt, for the power to control the line from Ogden to San Francisco, called the Central Pacific, was in itself an enormous asset, worth the value of this debt. Failure to pay this debt would have meant loss of control of the most valuable single factor in the great railroad system.

The claim of the United States was secured by a second mortgage on the Central Pacific. It was supposed that it would be sold to satisfy the first mortgage, and that it would realize no more than this sum, leaving, as a railway manager cynically expressed it, nothing but "two streaks of rust and the right of way." The government proposed, by a sort of injunction, to hold up the Stanford property, which would then be seized, in case the Southern Pacific Railway system should at some future time be found in debt. There was no warrant in law or in good policy for this suit. One United States judge spoke of it as "the crime of the century." It is not easy to work out the motives,

political or personal or what not, which inspired it. Fortunately, just now it makes no difference.

The hardest feature of the matter lay in the attitude of those jointly interested in the ownership of the Southern Pacific System. These men declined to give any assistance in the struggle for justice and for the endowment of the university. All were financially concerned in the final outcome, but they left her to make the fight alone and at her own cost.

It should be said that none of the present owners or managers of the Southern Pacific were in any way concerned in this matter. It is also fair to say that this attitude was only the business man's point of view. It seemed impossible to save the estate and the university together. All receipts of the railroads (there were no profits) were needed to continue its operations, and the outlays of the university seemed to the other owners of the railway system to involve a dangerous policy. On the other hand, to Mrs. Stanford the estate existed solely for the benefit of the university. To save the estate on these terms was to her like throwing over the passengers to lighten the ship. And as matters turned out, the university, the estate and the railway were all saved alike.

Perhaps we can get at the nature of this suit from a couple of letters written at the time. I find on our files a letter sent in November, 1894, to President Eliot of Harvard. In this letter I said:

> I recognize of course that public sentiment can not be formed without a basis of knowledge. The peculiar conditions in which this university finds itself are not easily stated to the public. There are internal reasons why we can not well take the country into confidence. Some of these reasons are connected with the relations of the Stanford heirs. Others arise from our relations to our future partner, in whose power we are, until the government suit is disposed of, that is, until the settlement of the estate.
>
> The grounds of the government suit, in brief, are these. The Central Pacific Railroad was regarded as an impossibility by most of the people of California. Its builders exhausted their funds and their credit and tried in vain to get help from every quarter, even after receiving large donations of land then worthless. The U. S. government came to their aid, whether wisely or not, . . . it does not matter at present. The road when finished bore a first mortgage, covering all that it is now worth. The government took a second mortgage upon it as security for the payment of the debt due for the bonds it had advanced in aid of the corporation. . . .
>
> There is a law in California, by which the original stockholders in a corporation are personally liable for its debts, if suit be begun within three years after the organization of the corporation. This law was intended to check "wild-cat" speculations.
>
> It is claimed that under this law the estates of Stanford and Huntington are still liable for the amount of the second mortgage, to come due in a few years. It is claimed that the three-years' limitation does not hold against the government. This question of liability had not been raised when the estates of the two remaining partners were distributed, and its enforcement would be possible as against the Stanford estate alone, as Mr. Huntington, being alive,

can withdraw his interests to Mexico, should the suit against Mr. Stanford be successful. Meanwhile, by the way, the question is tested for him at the expense of the Stanford estate, the railroad interests of which are in his hands as president of the road. . . .

It is believed by all jurists whom we have consulted, that the government has no case. The limitation of three years being an integral part of the statute in question, must hold against the government as against others. Furthermore, the aid extended by the government was not a debt incurred in business of the corporation.

However this may be, the courts will decide justly. Our anxiety is that they may decide speedily.

As to the various criticisms which you mention, permit me a word. In all personal matters, Mr. Stanford was perfectly truthful and just. Except in matters pertaining to the division of the earnings and bonds of the Central Pacific and the fact that its affairs were not made public, I have never heard his railroad career seriously criticized. In California, he had a very wide following among the best men, men who liked and respected him, not on account of his wealth and railroad connections, but rather in spite of them. In all the railroad war through which this state is passing, no responsible person has uttered a slur against Mr. Stanford or against the university.

It is not true that Mr. Stanford pretended to give the university a dollar more than he gave. He gave the three farms, formerly valued at $5,000,000, in these times worth much less; all the movable stock upon them, about $1,000,000 more; the university buildings costing $1,250,000; and by will $2,500,000 in cash. It was agreed by Mr. and Mrs. Stanford that each should be the residuary legatee of the other, and that whichever should survive should devote the rest of his or her life and estate to the university. The Stanford estate is therefore the university's endowment. Not in law but in fact the estate is the university. It was Mr. Stanford's feeling, and I was fully aware of it, that should his wife survive him, she should be free to endow the university and to control it as he had done. No one has ever struggled more loyally to do so than Mrs. Stanford. Since her husband died we have not received a dollar of his money, but the university has gone on without check or hindrance, though at times she has been forced to give up luxuries and to limit her expenses in every conceivable way. As a matter of fact, she has each year given me a personal bond for all she thinks that she can raise from the farms and from her own small personal property. Her devotion to the work is absolute and she is giving her life to it. When she loses, she will die.

The lands are unsalable only because the deed of gift prohibits their sale. In Mr. Stanford's lifetime they were conducted as parks. When they came into our hands, their products fell short by $10,000 to $20,000 per month of meeting the pay-rolls. This year under Mrs. Stanford's direction, they have yielded upwards of $150,000 above expenses. The sale of colts is a source of revenue now that the reputation of the Palo Alto stud is made.

No cash has ever been set aside in advance, for very simple reasons. I could not ask for it. Mr. Stanford was not expecting sudden death, financial panics, nor an attack from the government. He paid in cash all salaries and all bills, placed no limits on me, and on his sudden death left no debts against the university. There are now no debts left against his estate, which is appraised at $17,000,000, except the government claim which acts as an injunction tying everything up. It is not true that Mr. Stanford tried to "rear a personal monument by a good use of ill-gotten money." No one ever gave money in a more generous spirit, and there have not been many great givers who placed so

few restrictions on their gifts. Personal vanity does not give without restrictions in its own interest. He claimed that no man in California was the poorer for his wealth, which was true. It never occurred to him that it was "ill-gotten" or needed any apology.

I know better than any one else, except his wife, can, how genuine Mr. Stanford's interest was. He treated me, and through me, the university, with perfect truthfulness and justice. For my part and that of the faculty, we have tried to make the fund in our possession, count every dollar for a dollar to the best advancement of higher education.

As to the public at large, in time they will judge us by our fruits, if we are allowed to live to bear fruitage.

To a loyal friend of Governor Stanford, Senator Hoar of Massachusetts, I wrote this on June 20, 1894:

You will pardon me for writing to you to express my very great pleasure and that of Mrs. Stanford in the stand you have taken in defence of Senator Stanford's memory and in the effort you have made toward the protection of the university from the evil effects of prolonged litigation in which its endowment would be at stake.

You who knew Senator Stanford well know that the recent attack of Mr. Geary on his motives was without foundation in fact. The feeling of revenge at any real or supposed slight on the part of the legislature in connection with the State University, had nothing to do with his actions. He was not a man to cherish that kind of feelings. The sole basis that accusation had was this: Mr. Stanford acted for a few days as a member of the State Board of Regents. He was very much surprised to find that this board ignored the recommendations of the president of the university, and in general were disposed to treat the university chairs as personal "spoils." This led Mr. Stanford to doubt whether, if he should endow a university for California, it would be wise to place it in the hands of a political board of regents. These conditions in the State Board have now changed for the better. Mr. Stanford always spoke most kindly of the State University. He frequently consulted with its professors and it was a great pleasure for him to know that the new institution has in every way helped the old one. The friendly rivalry has been most salutary to both. Instead of 450 college students in one school as in 1890, there are now 1,700 students in the two, besides the professional classes.

As a matter of fact, Mr. and Mrs. Stanford founded the university with the sole purpose of putting their fortune to the best use of their country. I know Mr. Stanford's motives in this regard as well as one man can know the motives of another, and I know that no feeling of revenge and no selfish feeling entered into these motives.

The university has now safely passed every other serious difficulty. Mrs. Stanford has no other purpose in life than that of carrying out every detail of her husband's purposes. Her devotion has shown itself in maintaining the work of the university unimpaired during this period of hard times, while the estates are in probate, and therefore not available for university purposes.

It would, I believe, be a great national calamity if this great fund were lost to higher education. It would be almost as great a calamity if it were exposed to the delay and loss of prolonged litigation.

I assure you that the great majority of the self-respecting people of California are very grateful to you for what you have done towards the protection of the university endowment.

The story of the passing of the great suit is known to all the old students of the university.

It was brought to trial in San Francisco in the United States District Court, and the university side of the question had the strong support of the great jurist, John Garber.

The decision of Judge Ross was against the claim of the government. It was appealed and came before Judges Morrow, Gilbert and Hawley, who again found no merit in the government contention. It was appealed to the Supreme Court of the United States, and here our case seemed hopeless. The Supreme Court moves slowly, and our life-blood was ebbing fast. It takes money to run a university, and our money was almost gone. To delay the matter was to destroy us, and no one but ourselves had any interest in pushing along the decision.

Finally Mrs. Stanford went to Washington to appeal to President Cleveland. She told him our story, and beseeched him to use his influence for a speedy settlement. Once for all, let us know the future and we will stand by it. At last, President Cleveland saw his duty, and through his influence the Stanford case was placed on the calendar of the United States Supreme Court for speedy trial. Joseph Choate, whose name every Stanford man should hold in grateful memory, supplemented the work of John Garber. The case came to trial, and by a unanimous decision, the work of Justice Harlan, Stanford University was again free!

The boys celebrated the victory as Stanford boys can. The United States Postoffice on the campus, a wooden shack now removed, was painted cardinal red, to its great improvement in appearance, and once for all and forever the future of the university was assured.

This was the end of the dark days, but not of the days that were difficult. There were still eight millions of dollars to be paid. There was still the uncertainty as to whether Mrs. Stanford could survive to pay it, and the estate must come into her hands before she could give it to the university. She made many attempts to facilitate this transfer. At one time, we have the pathetic figure of the good woman going to the Queen's Jubilee in London, with all her own possessions, half a million of dollars worth of jewels, in a suit case carried in her hand. She hoped to sell these to advantage, when all the world was gathered in London. But the market was not good, and three fourths of them she brought back to California again.

And this seems the appropriate place for the story of the jewel fund. It is told in an address made at the foundation of the Library Building, and again and finally in a resolution of the Board of Trustees.

On May 15, 1905, I said:

> There was once a man—a real man, vigorous, wealthy and powerful. He loved his wife greatly, for she, wise, loyal, devoted, was worthy of such love. And because among all the crystals in all the world the diamond is the hardest

and sparkles the brightest, and because the ruby is most charming, and the emerald gentlest—the man bought gifts of these all for his wife.

As the years passed a great sorrow came to them; their only child died in the glory of his youth. In their loneliness there came to these two the longing to help other children, to use their wealth and power to aid the youth of future generations to better and stronger life. They lived in California and they loved California; and because California loved them, as she loves all her children, this man said, "The children of California shall be my children." To make this true in very fact he built for them a beautiful "Castle in Spain," with cloisters and towers, and "red tiled roofs against the azure sky"—for "skies are bluest in the heart of Spain." This castle, the Castle of Hope, which they called the university, they dedicated to all who might enter its gates, and it became to them the fulfilment of the dream of years—a dream of love and hope, of faith in God and good will toward men.

In the course of time the man died. The power he bore vanished; his wealth passed to other hands; the work he had begun seemed likely to fail. But the woman rose from her second great sorrow and set herself bravely to the task of completing the work as her husband had planned it. "The children of California shall be my children"—that thought once spoken could never be unsaid. The doors of the castle once opened could never be closed. To those who helped her in these days she said: "We may lose the farms, the railways, the bonds, but still the jewels remain. The university can be kept alive by these till the skies clear and the money which was destined for the future shall come into the future's hands. The university shall be kept open. When there is no other way, there are still the jewels."

Because there always remained this last resource, the woman never knew defeat. No one can who strives for no selfish end. "God's errands never fail," and her errand was one of good will and mercy. And when the days were darkest, the time came when it seemed the jewels must be sold. Across the sea to the great city this sorrowful, heroic woman journeyed alone with the bag of jewels in her hand that she might sell them to the money changers that flocked to the Queen's Jubilee. Sad, pathetic mission, fruitless, in the end, but full of all promise for the future of the university, founded in faith and hope and love —the trinity, St. Paul says, of things that abide.

But the jewels were not sold, save only a few of them, and these served a useful purpose in beginning anew the work of building the university. Better times came. The money of the estate, freed from litigation, became available for its destined use. The jewels found their way back to California to be held in reserve against another time of need.

A noble church was erected—one of the noblest in the land, a fitting part of the beautiful dream castle, the university. It needed to make it perfect the warmth of ornamentation, the glory of the old masters, who wrought "when art was still religion." To this end the jewels were dedicated. It was an appropriate use, but the need again passed. Other resources were found to adorn the church—to fill its windows with beautiful pictures, to spread upon its walls exquisite mosaics like those of St. Mark, rivaling even the precious stones of Venice.

In the course of time the woman died also. She had the satisfaction of seeing the buildings of the university completed, the cherished plans of her husband, to which she had devoted anxious years, fully carried out. Death came to her in a foreign land, but in a message written before her departure to be read at the laying of the corner-stone of the great library, she made known

the final destiny of the jewels. She directed that they should be sold and their value made a permanent endowment of the library of the university.

And so the jewels have at least come to be the enduring possession of all the university—of all who may tread these fields or enter these corridors. In the memory of the earlier students they stand for the Quadrangle, whose doors they kept open, and for the adornment of the church, which shall be to all generations of students a source of joy and rest, a refining and uplifting influence. To the students who are to come in future days the message of the jewels will be read in the books they study within these walls and the waves of their influence spreading out shall touch the uttermost parts of the earth.

They say there is a language of precious stones, but I know that they speak in diverse tongues. Some diamonds tell strange tales, but not these diamonds. In the language of the jewels of Stanford may be read the lessons of faith, of hope and good will. They tell how Stanford was founded in love of the things that abide.

It was resolved by the Board of Trustees on May 29, 1908, as follows:

WHEREAS, it was a cherished plan of Mrs. Jane L. Stanford that all jewels left by her should be sold after her death, and that the proceeds (estimated by her at more than five hundred thousand dollars) should be invested as a permanent fund, of which the income should be used exclusively for the purchase of books for the Library of the Leland Stanford Junior University; and

WHEREAS, the pressing financial needs of the university compelled her temporarily to forego said plan, and to sell many of said jewels in her lifetime in order to raise money to maintain the university; and

WHEREAS, by communication delivered to this board at its meeting, held February 22, 1905, Mrs. Stanford declared:

"In view of the facts and of my interest in the future development of the University Library, I now request the trustees to establish and maintain a library fund, and upon the sale of said jewels, after my departure from this life, I desire that the proceeds therefrom be paid into such fund and be preserved intact, and invested in bonds or real estate as a part of the capital of the endowment, and that the income therefrom be used exclusively for the purchase of books and other publications. I desire that the fund be known and designated as the "Jewel Fund." I have created and selected a Library Committee of the Board of Trustees, under supervision of which all such purchases should be made."

NOW, THEREFORE, in order to carry out said plan of Mrs. Stanford and to establish and maintain an adequate library fund, and to perform the promise made by this board to her, it is

Resolved, that a fund of five hundred thousand dollars, to be known and designated as the "Jewel Fund" is hereby created and established, which fund shall be preserved intact, and shall be separately invested and kept invested in bonds or real estate by the Board of Trustees, and the income of said fund shall be used exclusively in the purchase of books and other publications for the Library of the Leland Stanford Junior University, under the supervision and direction of the Library Committee of this Board of Trustees.

It was in these dark days that I was asked by President Cleveland through Mr. Charles S. Hamlin, to go to Bering Sea to help settle the fur seal disputes.

Before I started, in 1896, Mrs. Stanford said: "Now that our af-

fairs are looking so much better, do you not think that I might afford to bring back my housekeeper?" Her servants then were her secretary, her Chinese cook, and an old man, a servant of other days, who served as butler, without salary.

It was in these days, too, that Mrs. Stanford, going to Washington to settle up the household affairs of the mansion occupied while Mr. Stanford was senator, took four hundred dollars with her, lived in the private car owned by the Governor, attended to the packing of her goods, and the rental of her house to a senator from New York, and brought back $340 of the amount, which she turned over to me, to be used for the university. I have given this and other details private and personal, but full of meaning as showing her devotion to the university, and her utter unselfishness in carrying out the plans made by herself and her husband for the welfare of the men and women of the coming generations of California and of the world. While matters inside the faculty and the details of instruction were left to those supposed to be experts in these lines, for this was her husband's wish, she had always before her his purposes. "What would Mr. Stanford do under these conditions?" was always her first question; and in almost every instance this question led to a wise decision.

To outside suggestions as to this or that, she used to reply: "I will never concern myself with the religion, the politics or the love affairs of any professor in Stanford University." And this resolution she religiously kept.

With the passing of the government suit, conditions looked brighter. The payment of the eight millions went on very slowly, because the railway holdings could not be broken and must be sold as a whole if at all. The taxes on properties yielding no income became an intolerable burden. Besides, it was apparent that the original enabling act under which the Board of Trustees was organized contained grave defects, which might invalidate the actions of this Board. For this reason, mainly, the Board of Trustees existed in name only, Mrs. Stanford being in fact the sole trustee.

In 1899 the railroad holdings were sold, to good advantage, thanks to the good offices of a well-known German banker whose name I am glad to speak, James Speyer, and the estate at once passed out of debt. Finally, piece by piece, it passed into Mrs. Stanford's hands, and each piece was at once deeded to the Board of Trustees. The Board of Trustees was legalized by a change in the State Constitution. The university was by the same means relieved of part of the burden of its taxes. At the earliest possible moment, Mrs. Stanford again and in full transferred the whole estate to the board, reserving for herself a relatively small sum "to play with" as she said, but in fact to give her occupation and means to carry out in her own way other plans of strengthening the university and of helping mankind. The Board of

Trustees was then organized as a working body. Mrs. Stanford became its president, and this history passes over into the bright days of the dawn of the twentieth century.

Mrs. Stanford then left the university for a trip around the world by way of Australia and Ceylon. This was not that she wanted to see the world, or to be absent from her beloved Palo Alto, but that she wished to give to the Board of Trustees absolute freedom in taking up their great responsibilities. She wished them to handle the accumulated funds on their own initiative, without suggestion from herself.

The rest of the story can be told by others, for it is an open record. The whole may be summed up in these words of Mrs. Stanford in a letter written to me September 3, 1898:

> Every dollar I can rightfully call mine is sacredly laid on the altar of my love for the university, and thus it ever shall be.

That all this may seem more real, I venture to quote a few paragraphs from personal letters of Mrs. Stanford written in the dark days from 1893 to 1899.

On November 24, 1895, Mrs. Stanford wrote from the university:

> It has been my policy to say as little about my financial affairs to the outside world as possible, but I feel sure that I am doing myself and our blessed work injustice by allowing the impression among all classes to feel certain there is plenty of money, at my command, the future is assured, the battle fought and won. . . . I only ask righteous justice. I ask not for myself, but that I may be able to discharge my duty and loyalty to the one who trusted me, and loved me, and loves me still. I am so poor myself that I can not this year give to any charity; not even do I give this festive season to any of my family. I do not tell you this, kind friend, in a complaining way, for when one has pleasant surroundings, all we want to eat and wear, added to this have those in their lives we can count on as friends, it would be sinful to complain. I repeat it only that you my friend may know, I ask only justice, to the dear ones gone from earth life and the living one left.
>
> I am willing you should speak plainly to any one who may question as to the university or myself. I have many devoted and true loyal friends in Washington, and I am sure did they know I was kept from my rights, they would speak their sentiments openly, and when it was known a public sentiment was in my favor and against their unfairness, it would cause a different course to be pursued toward me. I shall henceforth speak plainly, and I desire you to do so. You will meet our good President, Mr. Cleveland, my good and true friend Secretary Carlisle, Mr. John Foster and many others, and you . . . can do our blessed work good and God will bless the act, and bring fruit to bear from the seeds sown. I have kept myself and my affairs in the background. It has been an inspiration from the source from which all good comes, from my Father God—I trust Him to lead me all along the rest of the journey of life. He has led me thus far through the deep waters, and joy will come, for He never deserts the widow, the childless, the orphan. I have His promise "blessed are those who mourn, for they shall be comforted."

On the same day she said:

> Everything is going on smoothly as far as I know at the university. The

boys are wild over the game to be played. I hope they will win because my boys will be happy if they win.

On July 20, 1896, she wrote to a candidate for a professorship:

The university still is restricted and limited in its ambitions and its aims, because of my inability to increase the number of students or the number of professors. The gift of $2,500,000 in bonds which I have by the grace of God been enabled to give to the trustees for the present and future maintenance of the university brings in a monthly income of $10,000, while the actual expenses for the faculty and the president and the necessary matters bring the sum total of expenses per month to $19,000. This $9,000 I am obliged to furnish myself, through the strictest economy and the husbanding of resources; consequently I am not increasing expenses but on the contrary shall retrench in the future.

On December 28, 1895, she said:

I must confess to a feeling of great pride in our entire body of students, both male and female, and I think we are all in a way under obligations to them for their uniformly good conduct, and a desire, as my dear husband once expressed it, to be ladies and gentlemen.

On July 29, 1895, she wrote:

I send a precious letter from Mr. Andrew White for you to read. I read it with a heart running over with various emotions. Mr. Stanford esteemed him so highly I could not but feel like asking God to let my loved ones in heaven know the contents of this letter. I prize this letter beyond my ability to express. It lifted my soul from its heaviness. My heart is one unceasing prayer to the Allwise, All Merciful one, that all will be well for the future of the good work under your care. When the end of our troubles is over, all (these letters) will be placed in your hands for future reading by our students, a story for them when I have passed into peace.

Soon after, she wrote:

I return herewith Mr. Choate's kind letter. God bless him, for he was a friend indeed.

After the decision of Judge Ross (July 6, 1895), she wrote:

I dare not let my soul rejoice over the future. It must be more sure than it is now. I hope and pray that the final decision will be as sure as the first. It means more to me than you or the world have dreamed. It means an unsullied, untarnished name as a blessed heritage to the university. My husband often used to say: "A good name is better than riches." God can not but be touched by my constant pleading, and this first decision by Judge Ross makes me humble that I so unworthy should have received the smallest attention.

From Paris, August 30, 1897, she wrote:

I wish the rest of my responsibilities caused me as little care as does the internal working of the good work. I am only anxious to furnish you the funds to pay the needs required. I could live on bread and water to do this, my part, and would feel that God and my loved ones in the life beyond this smiled on the efforts to ensure the future of my dear husband's work to better humanity.

Again, in 1897, she writes to her trusted solicitor, Russell Wilson:

I stand almost alone in this blessed work left to my care, and I want and

need the president's support and his helpfulness in this work as far as he can support me. There are plenty who are interested in the affairs of the estate with me, but few in the university.

In July, 1898, she said:

If I am able to keep the university in the condition it is now, I shall be more than thankful. $15,000 a month is a great expenditure, and exhausts my ingenuity and resources to such an extent that had I not the university so close to my heart I would relieve myself of this enormous burden and take rest and recreation for the next year. But I prefer to see the good work going on in its present condition, and I am not promising myself anything further for the future until the skies are brighter than they are now.

On December 14, 1900, she repeats:

I could lay down my life for the university. Not for any pride in its perpetuating the names of our dear son and ourselves, its founders, but for the sincere hope I cherish in its sending forth to the world grand men and women who will aid in developing the best there is to be found in human nature.

These extracts, largely from business letters, will show better than any words of mine her spirit and her faith. These must justify and make live the words I used on February 28, 1905, the date of Mrs. Stanford's sudden death in Honolulu.

The sudden death of Mrs. Stanford has come as a great shock to all of us. She has been so brave and strong that we hoped for her return well rested, and that her last look on earth might be on her beloved Palo Alto. But it was a joy to her to have been spared so long; to have lived to see the work of her husband's life and hers firmly and fully established.

Hers has been a life of the most perfect devotion both to her own and her husband's ideals. If in the years we knew her she ever had a selfish feeling, no one ever detected it. All her thoughts were of the university and of the way to make it effective for wisdom and righteousness.

No one outside of the university can understand the difficulties in her way in the final establishment of the university, and her patient deeds of self-sacrifice can be known only to those who saw them from day to day. Some day the world may understand a part of this. It will then know her for the wisest, as well as the most generous, friend of learning in our time. It will know her as the most loyal and most devoted of wives. What she did was always the best she could do. Wise, devoted, steadfast, prudent, patient and just—every good word we can use was hers by right. The men and women of the university feel the loss not alone of the most generous of helpers, but of the nearest of friends.

To these words spoken when the shock of the death of the mother of the university first came to her children, I added later a single thought as to Mrs. Stanford's conception of the future development of the university.

It should be above all other things, sound and good, using its forces not for mental development alone, but for physical, moral and spiritual growth and strength. It should make not only scholars, but men and women, alert, fearless, wise, God-fearing, skilled in "team work" and eager to "get into the game," whatever the struggle into which they may be thrown. To this end she would

have the university not large but choice. There should be no more students than could be well taken care of, no more departments than could be placed in master hands, no teachers to whom the students could not look up as to men whose work and life should be an inspiration to them. The buildings should be beautiful, for to see beautiful things in a land of beauty is one of the greatest elements in the refinement of clean men and women. Great libraries and great collections the university should have, but libraries and collections should be chosen for their fitness in the training of men. And with all the activities of athletics, of scholarly research, of the applications of science to engineering, the spirit of "self-devotion and of self-restraint," by which lives have been "made beautiful and sweet" through all the centuries should rise above all else, dominating the lower aspirations and activities as the great church towers above the red tiles of the lower buildings. But for all this, the Church should exist for men—for the actual men who enter its actual doors—not men for the Church. For this reason, any special alliance with any of the historic churches of Christendom is forever forbidden.

We do not yet see all these things. Rome was not built in a day, nor Stanford in a century. But as the old pioneers returning now behold in solid stone the dream-castles of their college days, so shall you, Stanford men and women, find here as you come back to future reunions, the university of your dreams, the university of great libraries and noble teachers, the university of the perfect democracy of literature and science, "of self-devotion and of self-restraint," the university in which earnest men and women find the best possible preparation for work in life, the university which sends out men who will make the future of the republic worthy of the glories of its past, the university of the plans and hopes of Leland Stanford, the university of the faith and work and prayer of Jane Lathrop Stanford.

LIFE FROM THE BIOLOGIST'S STANDPOINT[1]

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THE data of biology are living plants and animals. These are what nature presents. To these we must always go in order to make a beginning at any investigation. Is one interested in ganglionic cells, or germ cells, or liver secretions, or degenerate organs? He must find some kind of animal that has, or produces, or can yield such things. In making a successful quest for "material," it always turns out that a particular individual plant or animal, one or more, furnishes it. One may not be able to tell exactly what he means by an individual tree or man, but he must have one before he can study it or any part of it. Definitions of natural objects come at the end, rather than at the beginning, of our knowledge of them.

We biologists frequently speak of the principle of life, or the germs of life, and of many other particular manifestations of organisms, as though they were something really existent independently of particular organisms. Such questions as: Which came first, or is more fundamental, the chick or the egg; structure or function; life or organization? are frequently asked with more or less seriousness. Herbert Spencer devotes considerable space to the inquiry as to whether life or organization appeared first. He writes:

> It may be argued that on the hypothesis of Evolution, Life necessarily comes before organisation. On this hypothesis, organic matter in a state of homogeneous aggregation must precede organic matter in a state of heterogeneous aggregation. But since the passing from a structureless state to a structural state is itself a vital process, it follows that vital activity must have existed while there was yet no structure: structure could not else arise. That function takes precedence of structure seems also implied in the definition of Life.

He continues:

> If Life is shown by inner actions so adjusted as to balance outer actions

[1] During the academic year 1908–9 the program of the Philosophical Union of the University of California consisted of a series of discussions led by speakers representing various departments of biology and framed in a spirit compatible with the broad aims of such an association. This was the concluding paper of the year.

Wir denken heute durchweg more biologico. . . .

. . . dass die Biologie selbst heute noch im Zustand des gärenden Werdens, der tastenden Unsicherheit sich befindet, also für eine Grundlegung der sichersten aller Wissenschaften, der formel Logik, noch keine Eignung besitzt, begeht der Pragmatismus denselben Circulus vitiosus dem auch Hume nicht zu entrinnen vermochte. . . .—*Ludwig Stein.*

. . .; then may we not say that the actions to be formed must come before that which forms them . . . that the continuous change which is the basis of function, must come before the structure that brings function into shape?[2]

Greatly to Mr. Spencer's credit he tells us in another connection (p. 197), that "in truth this question is not determinable by any evidence now accessible." We must go a long way beyond this position and recognize that not only is the question not determinable "by any evidence now accessible," but that there is not the slightest indication that such evidence ever will be accessible. What we have to see is that all such discussions are utterly futile for science; indeed, that they have no legitimate place in inductive science.

Has anybody ever seen an egg that was not produced by some organism; some function without structure, or *vice versa;* some life without organization, or organization independent of life? Surely not. Then equally surely you can make no assumption that involves the disjunction of either member of one of these couples from the other, without attempting to transcend experience—without becoming in so far an *a priorist* pure and simple.

Now you may perhaps have the privilege of being an *a priorist* pure and simple, if you want it, but in case you choose thus you can not have a seat in the temple of physical science for one instant. On the basis of experience science can project itself far in advance of experience, but only on that basis can it thus project itself.

So much for the data, the starting places of biology. They are *individual animals and plants, living in nature.* It is wholesome for any domain of science to stop now and then and ask what its original data are. Such inquiry not only yields enlightenment, interesting and useful of itself, but it is further illuminating as to the way a science deals, and *must* deal, with its raw material—its "givens."

Notice the procedure in a special case. Observe how oceanography proceeds in studying the Pacific Ocean. Of what is that vast sea composed? First of all of water, H_2O. No doubt about that. Dissolved in this are various mineral salts, chlorides of sodium and magnesium, particularly, and the gases O, N and CO_2. These with perhaps a few other elements and the ocean is chemically accounted for. Yet how far have we gone toward a knowledge of the Pacific Ocean when we have found that it is thus constituted? Even though we should have ascertained the total quantity of water, salts and gases in the entire Pacific, we should have scarcely made a beginning on the oceanography of this body of water. Its form and boundaries; its connections with other oceans; the character of its bottom; its islands, continental and oceanic; its currents; its tides; the up-welling waters on its eastern margins; its temperature in general, and in particular parts, and dozens of other matters, are quite over and beyond anything

[2] "Principles of Biology," Vol. I., p. 210.

that strict chemical knowledge can reach. Oceanography as now understood is quite impossible without chemistry, but it by no means follows that chemistry is the whole of oceanography. Physics is as essential as chemistry; and geology and astronomy are in turn as essential as physics.

So with the other inorganic sciences. Spectroscopy, a department of chemistry, has been largely the making of modern stellar astronomy. Yet is not such a problem as that of the variable stars, something over and above spectroscopy? Is it conceivable that spectroscopy alone would ever have *discovered* variable stars, and formulated the many interesting questions about them that astronomy is now asking?

Do not the same principles of constitution, and study of constitution, hold when we enter the domain of living objects? They surely do. Organisms have their own special qualities and so present their own problems, exactly as do oceans and stars. Biology depends upon, but at the same time transcends, chemistry and physics, in exactly the same way that astronomy rests upon but transcends chemistry and physics. We are here on the threshold of one of the oldest, in many of its aspects one of the most familiar scientific and philosophic puzzles; namely, that of the relation of a whole to the elements which compose it.

Alas for the proneness of humankind to go all awry with itself and nature from not duly heeding the commonest, most familiar things! Hear this dialogue that comes to us across a stretch of two thousand years:

Socrates—"Suppose one were to ask you a question about the first syllable in the name Socrates, and say 'Theætetus, tell me what SO is,' what would you answer?"

Theætetus—"That it is S and O."

S.—"Well, have you not there the reasoned statement of the syllable?"

T.—"Yes, certainly."

S.—"Proceed then and give me in the same way the reasoned statement of S."

T.—"But how can one give the elements of an element? For indeed, Socrates, S is one of the voiceless letters, a mere sound, as it were a whistling of the tongue. . . ."

S.—"But stay, I wonder if we are right in laying it down that while the element is not knowable the combination is? . . ."

T.—"That would be strange beyond all reason, Socrates. . . ."

S.—"Perhaps we ought to have taken the combination to be not the sum of the elements, but a single form resulting from them, with an individual shape of its own, and differing from the elements."

T.—"Certainly, very possibly this view is more correct than the other. . . ."

S.—"Then let the combination be, as we now put it, a single form, alike in letters and in everything else, resulting from the conjunction of harmonious elements in each case."[3]

[3] "The Theætetus and Philebus of Plato," translated by H. F. Carlill, in "New Classical Libary."

As long as the mind of the interpreter is human, the whole truth of a complex natural object or proposition can never be ascertained from knowledge of its components alone. Or varying this statement, you can *never give a full account of any whole in terms of its elements.*

In spite of the ocean and the stars as illustrating the truth thus formulated, the statement sounds dogmatic. We must examine it farther.

The presumption that biological phenomena may be adequately treated in terms of chemistry and physics takes care of itself so far as strict science is concerned, since its utter futility becomes apparent almost immediately it is put to rigid experimental test. For one thing, it results in constant effort to extend generalizations far beyond where later study will permit them to stand. It leads inevitably to a forcing of evidence, which process sooner or later comes to grief.

One aspect of this forcing is almost certain betrayal into an illegitimate use of the analogical mode of reasoning. For instance, an analogy is often drawn between the so-called reversed actions in chemistry, and what is spoken of as a return of certain animals—certain worms—to the egg state. As a matter of fact the earlier speaker who drew this analogy might have used the " second-childhood " of the old man as well as the supposed second egg state of the worm. One has as much in common as the other with the chemical process for which correspondence was claimed.

You must not understand by this that I condemn, wholly, comparison and analogy in reasoning. On the contrary, I attach great importance to these, as would become clear were this discussion to be carried into regions where it is not possible for it to go now. In so far as there is resemblance between reversed chemical action and growing old, the fact is illuminating, and to have discovered it is good. My criticism is directed not against pointing out the resemblance, but against not pointing out the difference at the same time, thus leaving the inference that one process accounts for or explains the other.

It is in its wider bearings, its bearings beyond strict specialties in science, that the influence of the theory of physical-chemical adequacy in the treatment of life phenomena is most unfortunate. Only when regarded from this larger standpoint does its withering effect on the scientific spirit and method generally, and on man's attitude toward nature, become apparent.

The subject is, according to my view, so vital that I must ask you to look into it more closely. This we can not do without running a little into what these walls are accustomed to hear about under the term theory of knowledge, or epistemology. Most of us would agree that we have to use both our senses and our minds in science. Most would agree too that that workman is the most efficient who uses his instruments the most intelligently—who is not a mere rule-of-thumb

workman. How then do our minds and our senses work while we do science? Are there general principles of operation, to know which would enable us to use them more smoothly, more surely, more productively? Let us watch them while they work at some problem of chemistry, say. To be as objective as possible we will use common table salt. We began with a general acquaintance with the article. How do we become thus acquainted? Surely in no other way than by examining it. We touch our tongues to it, it dissolves readily and has a characteristic taste. Examining this dissolving propensity minutely, we find that in distilled water at a definite temperature, a definite quantity will be taken up. Its *solubility* is thus determined.

The moment we handle it in considerable amount we note that it is rather heavy. In water it sinks quickly. This property we examine more closely and find that a *specific* gravity characterizes it.

In a pulverized state it is pure white. If, however, we let it evaporate slowly, we get cubical crystals, not white but transparent.

We may suppose now we have examined all the *physical* properties of salt. But surely our knowledge is not yet complete. We know nothing of its composition. Before beginning on this chemical extension of our knowledge, let us take due note of the fact that table salt is a definite thing to us; we can use it in a hundred ways and rely implicitly upon it, on the basis of this physical knowledge alone. We do not have to know whether it is simple or compound in order to get its benefits *as salt*. *Physical* knowledge of it we must have before we can use it in any way. *Chemical* knowledge, understanding by this knowledge of constituents, we need not have.

But since we started out to know salt through and through, we must become salt chemists. We must decompose the substance, if it turns out to be compound, and examine its constituents as carefully as we examined the substance itself. To make the story short we get sodium and chlorine. But we must not so shorten the story as to fail to see what we do in examining these constituents. What is it that we do? We proceed exactly as we did in examining the salt. We determine their physical properties. The sodium is opaque, and bright metallic in color. Ordinarily it is amorphous and waxy. Instead of dissolving in water as does the salt, it decomposes water. Instead of sinking quickly, it floats. It melts at 95°.6 C., while 776° of heat are required to melt salt.

The chlorine, a gas at any temperature we can readily command, is greenish-yellow in color. Its odor is characteristically disagreeable, and it irritates our noses and throats. Like the salt it is soluble in water, but while the salt is more soluble in hot water than cold, chlorine is more soluble in cold water. It is heavier than air, though much lighter than water.

What is the net result of our examination of the constituents of

salt? First and foremost an interesting lot of entirely new knowledge. Our understanding of salt has been broadened and deepened. Salt is a much more complex thing to us now than it was before. Instead of simplifying salt by reducing it to its elements, we have greatly complicated it. But notice this particularly: The new knowledge has not enhanced by one jot our knowledge of the *physical properties* of salt. There is nothing whatever in the properties of the sodium or the chlorine that gives us any clue to the properties of the salt. We might, so far as the best cunning in observation we now possess promises anything, examine the sodium and the chlorine till doomsday and never suspect that together they might produce salt, unless we happened to put them together and note that salt actually did result.

This is a threadbare, school-book story. Why revamp it here? Because it is part, though an essential part, of a much larger story, the whole of which is rarely if ever told. Before we can reach the heart of the matter we must stop a moment with another fact so familiar as likewise to seem stale. Our physical examination of the salt, the sodium and the chlorine, were applications of the general principle that the first step in all knowledge of external objects is the determination of their physical qualities. The familiar expression is, "we know an object only by its properties, or qualities." Let us take this statement from its pigeon-hole of mere habit, and look at it reflectively. Does it mean that there are no natural objects in all the universe about which we can get knowledge in no way other than through their physical properties? Those of you who say "yes, that is what it means," I agree with, and with you might go on at once with the discussion. But some will, I suspect, hesitate to reply thus. To you who hesitate, I say that if there be objects which we may know by other means than through their physical properties, they must be namable, otherwise you could not claim for them a place in the physical world; so I demand that you mention examples. You will probably name the atoms of the chemist and the ether of space. You are then, in so far as concerns the atoms, committed to the conception of propertyless atoms, are you not? I ask you to tell me then exactly what it was that John Dalton and the other founders of modern chemistry actually did. Certainly there were atomic theories of the constitution of matter long before these men lived. Democritus and Newton, to say nothing of others, made much of such atoms. Why did the pre-Daltonian atoms signify nothing, or almost nothing for physical science? *Because they were propertyless atoms.* To attach to these old purely speculative, and hence scientifically useless atoms, *one property* of the particular substance to which they belong, was exactly what these chemists did. The property so attached was that of *combining with other substances in definite ways.*

Analyze the atomic theory of modern chemistry and you will find

that all that makes it significant for chemical practise is expressible in this way: If any two substances unite with each other chemically, they do so in such a manner that there are no particles of them so small as not to follow the same law of combination that holds for the substances in bulk.

Atoms in modern chemistry are small bodies imagined to constitute visible substances, and they are imagined for the purpose of incarnating, if you will, the observed trait that substances have of combining with one another according to known rules. As to most of the other properties of atoms, their shape, color, hardness, etc., if atoms are conceived to be anything else than little particles of the substances, science knows no more to-day than did Newton and Democritus.

The purpose of this little excursion into the atomic doctrine, that border-land of physical science, is to bring home something of the mighty power there is in the properties of things, and in sense experience. It is not too much to say that the modern science of chemistry was born then and there, when one property that substances have, viz., that of definite combination with other substances, was attached to the hitherto purely speculative, more or less mystical atoms of those substances.

I ask you now to recall what was said about the way we deal with the salt, sodium and chlorine. Substantially the statement was that we have to treat them all on exactly the same basis, so far as the process of knowing is concerned. That is, we have to treat each one on the basis of *its own properties.* We can not touch sodium with our knowledge of the properties of chlorine, nor *vice versa.* Similarly, we can not touch sodium with our knowledge of the properties of salt, nor salt with our knowledge of the properties of sodium, except, mark you, as we may say that one property of sodium is its power to unite with chlorine to produce salt. My familiar expression for this is that the external world and our minds are so constituted, are so articulated with each other, that every object in that world must be treated *on its own merits.* Now notice that since these substances must be treated, each on its own merits, and since the sodium and chlorine have the power of combining with each other in such a remarkable way that they wholly lose their original properties, at least temporarily, and merge into another substance, salt, with properties wholly *its own,* we must recognize that the properties of substances manifest something of transitoriness and relativity.

Thus are we led to the notion which I have ventured to speak of as the *standardization of reality.* The expression is suggested by the chemist's process of standardizing solutions; the process, that is, of using a solution of known composition and concentration as a unit of value to which to refer various reactions and processes. The meaning is that whatever criterion of reality you apply to any natural object,

that same criterion you must apply to all other natural objects, no matter whether some of these be constituents of others, or stand in some other relation to one another.

Making the statement specific for the case of objects that are composed of other objects or substances, it runs thus: Whatever criterion of reality you apply as the test of the elements of a complex body or substance, exactly that same criterion you must apply as a test of the reality of the complex body itself.

It follows from this, that with the question of a fundamental essence or substance behind properties, we are, as students of objective nature, in no wise concerned. As to whether there is or is not a real essence of sodium, or of salt, to which the sensible properties of these substances adhere, is no affair of ours. Physical science can not even raise the question of an absolute reality or realities behind the objects with which it deals.

Now let us carry these considerations of the nature of objects and of minds, and the relation existing between them, up into the realm of objects that we call living. The formulary will run thus: In whatever sense you predicate reality, or fundamentality, or ultimateness to the germ or any part of an organism, in exactly the same sense you must predicate reality, or fundamentality, or ultimateness to the completed and whole organism.

If you have been accustomed to look upon living nature with the conception that somewhere deeply hidden in the plants and animals you daily meet there is something more real than the organisms themselves, something possessed of a potency wholly unique and mysterious as contrasted with that possessed by the visible beings; or, if you have regarded living beings as ejects of your own consciousness—if, I say, you have been wont to thus regard organisms, grasp fully this conception of reality and of measuring reality and you will find, I believe, that it will transform your world. It will increase your interest in every developed organism as contrasted with your interest in its germ, or any portion of the organism physical or psychical in almost direct proportion as the sensible complexity of the organism as a whole exceeds the sensible complexity of the germ or any part of the organism.

Here is the epistemological necessity for the conception of an "organism as a whole," the biological compulsion of which we speak later. The point is simply this: Every object in nature has some nature of its own. That is just what makes it belong to nature. Consequently there must be something about it which can not be fully accounted for by referring it to something else in nature. For if you could thus dispose of every natural object, nature would consume itself in explanation. You would have the case of the Kilkenny

cats psychologized. We come here, I imagine, upon what virile truth there is in the "Ding an Sich," the "Thing in Itself."

I can not restrain my interest from going where I believe reality to be. Contrariwise, I can not send it where I believe reality is not. Interest and attention, like natural forces generally, take the direction of least resistance; and the places of greatest belief in reality are those of least resistance for attention. With this psychological basis to go on, illustration will carry us forward more surely and steadily than further argument.

The very heart of that school of biology known as materialistic, or mechanistic, is its effort to interpret living beings by ascribing to invisible substances or bodies, located somewhere within the germ-cells and other cells of the body, reality and essentiality of a sort quite unique as contrasted with the visible substances, and the organisms themselves. Examine the program of this school attentively and you will see that it proposes *to "explain" or "express" those parts of animate nature about which we know most, observationally, in terms of those parts about which we know least, observationally.* It is undoubtedly a quasi-inductive, semi-mystical program.

The chemical materialist conceives certain compounds, never well-known ones mark you, enzymes for example, to be thus supremely endowed. The biological materialist on the other hand, ascribes this exalted rôle to imaginary, invisible living bodies hidden deep within the germ and other cells. Biology of the last three decades has bestowed mighty powers upon such bodies under the designation "determinants." Only those familiar with the technical literature of the science during this time can have any notion of the influence these bodies have had. What have been, and what are the effects of such conceptions on biological theory and practise? I mention only a few of these.

Nothing is more characteristic of the biological thought in highest repute to-day than its disposition to look down upon all those kinds of research not aimed at the elements of organisms—at "ultimate problems," as the expression goes. Most of those who labor in the biological vineyard but are not elementalists of some sort, will appreciate what I mean, for they will have personally felt the ban placed upon them by the dominant school. A few years ago one of our best known American zoologists, speaking from a position of national preferment in his science, reviewed comprehensively the present range of zoology, and did not hesitate to pass upon the labors of description and classification of animals as hardly worth while. In other words, he pronounced as not worth doing, the very things which an examination of the nature of the knowledge-getting process shows to be absolutely fundamental steps toward an understanding of living nature.

This zoologist, it is hardly necessary to say, is an elementalist at heart. Plants and animals, as nature presents them, are for him real in a way. They are of course what our naked, crass senses come in contact with, but the real essence of them, the thing we want to get at, lies far away in the germ-cells, in the chromosomes, in protoplasm. *There* is reality. There is the pole-star by which our compass should be set, according to his views.

The consistent elementalist can not care much for description and classification, for these depend in the first instance on "mere qualities," while he is concerned with essences. The elementalist's problems, like the pure intellectualist's, are always ultimate problems. For both anything this side of the absolute is only appearance.

There is prevalent among many influential biologists, unfortunately for practical ends, a tendency to esteem what are called "gross" anatomy and physiology as of no great scientific value. To the elementalist this is bound to be so. The structures and the activities of your bodies, as the anatomist and physiologist of the ordinary kind sees them, are not their fundamentally real structures and activities. These are deep hidden in the uttermost recesses of your members. They are "probably" your proteid compounds, especially your enzymes.

One of the best characterization-marks of elementalist biology is the expression "nothing but." What is the human brain? It is "nothing but" a vast multitude of ganglionic cells (9,200,000,000 in the cortex alone), if the answer comes from a cellular elementalist; or it is "nothing but" a still greater number of chromosomes, if the elementalist be of the consistently orthodox chromosomal persuasion.

And what are the so-called emotions of the human breast? In last analysis they are "nothing but" chemical substances in unstable equilibrium, or in some other state.

Ernst Mach, that prince of modern elementalists, quotes Litchenberg approvingly as follows: "We should say *It thinks,* just as we say *It lightens.* It is going too far to say *cogito* if we translate *cogito* by *I think.* The assumption, or postulation, of the ego is a mere practical necessity." What sort of necessity, if not practical necessity, do these people believe in? Seemingly it is theoretical or impractical necessity, or both.

The answer to those who hold such views is obvious: If you want to call yourself "It" why, go ahead. But I propose to call myself "I" and no power in heaven or on earth can compel me to call myself "It." I may not be able fully to define my "I." Surely I am not, for full definition comes at the end and not at the beginning of experiential knowledge. But however incomplete my definition be, here I am. "The proof of the pudding is the eating."

Why do the elementalists pin their faith to the invisible constituents of things rather than to the things themselves? Can it be that they

deliberately deceive themselves as to the place where reality is located? Surely this can not be so. Some misguiding agent or agents there must be, and they must be subtle, otherwise they could not succeed as well as they do with so many earnest people.

Let us see if we can detect any of these subtle misleaders. In order to walk sure footed, we must remain on the platform of objectivity. Let us go back to salt and its elements. If the properties of salt are not derived from the sodium and the chlorine, where do they come from? Does something wholly extraneous to the elements while they exist apart, come in at the instant of their union that bestows upon the salt its peculiar properties? In other words, is there a mystical somewhat in chemical affinity? Those of you who know anything of the history of biological theory will recognize that this brings us to the threshold of the vitalistic school. If the completed organism does not lie as potency and promise in the germ and its natural environment, then where does it abide? If the qualities of the organism are not thus derived, then indeed is there something in man not derived from nature, just as a time-honored school of philosophy asserts. But for biology this would be vitalism, and vitalism means a walled city with the gates locked and the keys lost beyond recovery.

Have we reached a city surrounded by such a wall? A wonderful city indeed we have come to, for in truth is it an eternal city. By no means, though, are its gates locked against us. We may enter with perfect freedom and wander through its streets and palaces as long as we live, even to the latest generations of those who follow us, always there to find that which is more interesting, more beautiful, more marvelous.

Being primarily a man of science and only incidentally an artist, I am privileged to be a bad artist, so may intepret my metaphor.

What I mean is, that while we can not see how the properties of the salt are potentially in the chlorine and the sodium; and how the qualities of the man are potentially in the germ-cells, we still have no grounds for supposing they are not there. If our knowledge of the chemical elements and of the germs were full enough, we should see how they produce the results which flow from them. Now here is the crucial point—if our knowledge were *full enough* we should see. But how full would "full enough" be? So far as the knowledge we now have enables us to answer, only *unlimited knowledge would be full enough*. If we are privileged to suppose we shall sometime be possessed of infinite knowledge; shall be, in other words, infinite beings, then but not till then, shall we understand how chlorine and sodium produce common salt, how the germ-cells produce a common man.[4]

[4] Readers acquainted with Hume's teachings about the relation of cause and

Nature is through and through infinite in her forms and processes, so it seems from the experiential knowledge thus far gained.

In just what ways science is being driven to the conclusion that nature is thus constituted is too long and hard a story to tell here. We can only glance at a few of its specially striking features. The atomic theory of modern chemistry contains several of these. By modern chemistry is meant chemistry since Dalton, Lavoisier and Avogadro; and especially since Lorentz and the electron idea came into it.

The special thing about the atomic theory that I call your attention to in this connection is the conception of change of valence of atoms now being discussed by some of the foremost chemists. According to this conception, the same atom may have different combining values under different circumstances. Do you not see without further comment what this suggests as to unrevealed potentialities of atoms? If the known facts of carbon-chemistry are such as to drive the chemist to suppose the atom of carbon changes from bivalency to quadrivalency and *vice versa,* what sober chemist will venture to place any limitation on the possibilities for further change of like nature not only in this but in other atoms?

Since we know absolutely nothing about the relation of the atoms in *living* substance, would it not be a reasonable hypothesis to say that the nature of that marvelous process called metabolism is due to just the fact that the atoms of carbon, nitrogen, hydrogen, oxygen, etc., are undergoing perpetual change of valence? I see no reason why we may not legitimately imagine even consciousness due to such a process.

Were such a hypothesis to be seriously taken, it would seem to follow that consciousness would have its roots wherever metabolism is going on. What an excellent starting point this would make for dealing with the perennial puzzle of how it is that the "mind influences the body"! The mind would then be part of the body.[5]

Another fruitful idea recently introduced into chemistry, and significant for the present point, is what is known as mass action. The essence of this, as my colleague Professor F. W. Cottrell expresses it,

effect will recognize that at this point I part company with the keen-minded Scotchman. It is not necessary, however, to go into the matter here.

[5] Since preparing this essay my attention has been called to the writings of Henri Bergson. From what I gather by reading a number of reviews of his works and from a glance through his "Matière et Mémoire," it seems certain that many of my positions are close akin to his, though our starting points have been so very different. Among other things, this suggestion as to the chemical foundation of consciousness would seem to fall in admirably with the views held not only by M. Bergson but also by Avenarius, that not the brain alone but the whole body is the seat of conscious life. (See "Subjectivism and Realism in Modern Philosophy," by Norman Smith, *The Philosophical Review*, Vol. 17, 1908, p. 138.)

is that the more opportunity for chemical action the particles of a substance have, the more they act; that is, the particles improve their opportunity, so to speak.

See again how pregnant of meaning this is for the potentialities of atoms. It means that they have capacities to act that are revealed only when conditions for them to act are presented. This reminds one strongly of the unused energies of men that Professor James has recently written about so luminously.

The one other phase of science to which allusion will be made under this head, belongs to the biological realm. It is the conception of the "organism as a whole" that for a number of years has been working its way into biology by sheer force of its own weight. The facts are such as to compel admission even though they are wholly inexplicable on the basis of current elementalist doctrines, and so are frequently ignored or scouted by biologists of that school.

An expression which, though extreme, still rightly presents the idea comes from the German botanist de Bary. He said "Die Pflanze bildet Zellen, nicht die Zelle bildet Pflanzen" (The plant produces cells, not the cells produce the plant). This is an over statement but is true in so far as it expresses the unescapable fact that the whole organism at any given moment, as well as its elements, is concerned in determining what it shall be in the next succeeding moment. A more exact expression of a particular phase of the idea is due to our foremost American student of the cell, Professor E. B. Wilson. He writes: "*We can not comprehend the form of cleavage* (cell-division) *without reference to the end-result.*"

Let us look at an instance of the working of this principle in the realm of political organization, where it is more openly displayed. The original thirteen colonies of our pre-national period, united into a compact under what was known as the articles of confederation. A corner stone of the union was that each state should keep inviolate its original powers and privileges. Under the governmental fiction of this compact, the Congress, it has been said, could recommend everything but could enforce nothing. The experiment was naturally a failure. After a period of "Strang und Gang" our nation with the federal constitution as its basis was founded.

Now recall some of the striking things that happened in this transition time. First of all, the hitherto individual, sovereign states had to give up some of both their powers and their possessions. The "western lands" claimed by the states, had been one of the most serious obstacles in the way of a closer union. First New York, then Virginia, yielded their claims to congress for certain guarantees to them in return. The other states followed. Afterward the congress erected new states in the territory thus acquired, and the old states modified their organic laws to conform to the new conditions. Shall

we say that the new creation, the United States, contained something underived from the original states and their conditions? Shall we deny that our republic was contained potentially in the thirteen original states? Surely not. But the processes of gestation and parturition by which the nation came forth profoundly modified the elements, the states. Only a wisdom practically infinite could have foreseen exactly what those modifications would be.[6]

Growth and organization everywhere in living nature work inward as well as outward. The processes turn back upon themselves and produce changes in the contributing elements. What the new creation will be, what modifications the elements will undergo, one can see beforehand partly, but never fully. Only infinite wisdom could see altogether. Notice under what conditions one's wisdom would enable him to predict the future absolutely. Would not these two conditions be essential: That his knowledge *of the past* should be absolute, and that the course of events, that is the laws of nature, should be absolutely trustworthy?

Observation with our senses, of law-abiding operations, performed by objects cognizable only through their own properties, is one way of

[6] The criticism has been made that in using the origin of the United States as an illustration of the centripetal action of the developmental process, I am resorting to the analogical mode of reasoning, the very thing I have objected to in another connection. Attention must be called to the fact that it was not the use but the illegitimate use of this method to which objection was made. I am not pretending that the reciprocating action as it takes place in either the animal body or the nation explains the process *in the other*. My point is that in both cases the developmental process manifests this peculiarity. There is a common element in the two developments. That is all I am insisting on. But this must be taken in connection with the principle insisted on with equal emphasis elsewhere, that each natural object has *its own* qualities and properties. The man and the nation have something in common as to their mode of development, but they also have something of difference. To ascertain the *differences* and the *traits-in-common* all along the line is exactly what the business of developmental biology is.

Those biologists whose creed is that explanation of nature consists in reducing her to a few simple principles will make wry faces if nothing worse at this. But until such biologists can be more successful than they have been so far, in preventing organic chemists from finding new compounds day by day, and in suppressing systematic botanists and zoologists who persist in hunting up new kinds of plants and animals, and new characteristics and varieties of old ones, I see no prospect of these wry faces changing to expressions of good cheer.

It may be unfortunate that the living world is so complex, was not constructed on "a few simple principles." But one thing seems well established: Nature can not be made simple by treating her on the theory that she ought to be so when as a matter of fact she is not. To say that a few principles can be found that are common to very wide domains of nature, and to deny that there are numberless other principles not so widely prevalent are very different propositions.

describing our knowledge of nature. But we should fall wofully short were we to be satisfied with such an account of it. We can reach the kernel of a more adequate account by way of that indispensable aid to scientific discovery known as hypothesis-making.

A few, only a few, men of science have proposed to eliminate hypotheses from science altogether. The best known of these eliminators is Wilhelm Ostwald. In the place of the hypothesis Ostwald would install what he calls the *protothesis*. And what is that? It is a "vorläufige Annahme." There you have it! A protothesis is a taking of something by running on ahead. Ostwald wants to get rid of hypotheses altogether and rely wholly on "Arbeit," on work, to make conquests in science. But see what his proposal comes to, taking his own words. He is going to do part of to-morrow's work to-day, even at this very instant. The mind forecasts. It outstrips its past and present experiences. That is the vital fact, and why quibble about how it shall be named?

All generalization is hypothesis, says M. Poincaré. Think about it and you will see the eminent Frenchman is right. Think about it further and you will see you can not move ahead in real science one inch without generalization. But for it you might possibly have co-ordinated experiences which by courtesy might be called knowledge. But such knowledge would be wholly without motive, and what rational being would care a snap for such knowledge!

We must not fail to notice how radically at variance this way of interpreting the mind's work is from Kant's way of interpreting it. Kantians speak of that which the "mind itself puts into nature." If something is really put into nature, that something must have been previously outside of nature. You can not put water into a dish that is already in the dish. What is that outside something? Where is the outside source whence it comes? Ask the unfortunate mortals of whom Laura Bridgman was an instance, who are deprived from tender infancy of their sense organs, whether they know of some source of knowledge wholly outside nature. These cases furnish indubitable evidence, so far as they go, that consciousness has no content till sense perception gives it some.

No, the mind does not put something into nature that was previously outside it. This however, it does do: It takes something from one part of nature and puts it into another part. We must allow that the mind really does put something into *any particular situation* that was not in that situation before. But that is quite different from allowing that it puts something into *nature as a whole* that was not before somewhere in *nature as a whole*. This brings us back to our standardized, or tested, or relative reality.

If we ask how or by virtue of what quality or force the mind does this running ahead, this transferring of something from one part of

nature to another, no answer is forthcoming any more than there is to the question as to how or by virtue of what quality or force it senses at all; or to the question as to how or by virtue of what quality the properties of salt are produced by the sodium and the chlorine. It may be galling to find that we must accept many things as by free grace, so to speak; but this does not alter the fact.

It is the penalty we pay for belonging to nature at all. If one is galled by the fact and so tries to escape it, the course open to him is that taken by the oriental occultist who sees the natural order as a clog to the nobler but invisible real order and hence as a thing to be got rid of as soon as possible.

At a few places in this discussion it has seemed as though the course we were on would drag our physical science back to the primal chaos of mere sensations and facts from which it seems to have come. In truth, though, now that we can look at the whole situation as from a hilltop, how thoroughly familiar, how reassuring, how in accord with the best, most fruitful endeavor of all the ages of human history it is seen to be. "Nur in der Erfahrung ist Wahrheit" (only in experience is truth), said Kant. Modify this to the extent of making it say "Ohne Erfahrung ist keine Wahrheit" (without experience is no truth), and can any but a sophisticated mind doubt its truthfulness?

If nature is as true to herself and to man as she seems; if the body of evidence gathered by centuries of laborious science to the effect that law and order do prevail throughout the universe; and if the universe is as inexhaustible in variety and power as experience indicates, then how securely we stand on the truth that struggled to expression in Saint Paul's prophetic words: "Faith is the substance of things not seen"!

There seems to be no question about what experience *alone* can do since there is no such thing so far as we can see. Nor is there any question about what faith *alone* can do since there is no such thing. Experience appears to exist because of or through faith, and faith to exist because of or through experience. So far as production has reference to the fact that something exists now that did not exist previously, experience and faith must, it would seem, be said to be generative inter se. We have no ground whatever for saying that either preceded the other in time. Even the simplest sensation, the starting point of experience, can not be conceived in any intelligible terminology that does not recognize it as belonging to some organism. What sane person would talk of a sensation absolutely independent of an organism? But an organism of the simplest imaginable sort[7] must still have some measure of fidelity, of faithfulness to

[7] This remark need not be interpreted to mean that the simplest conceivable organisms actually did begin in time. For my part, I am of opinion that biology has reached the point where the suggestions of such cosmically-minded men

itself; must have, for example, some measure of persistence or constancy in time. I am unable to imagine an organism existing but for a single instant.

The moment a living being appeared on earth that could *respond more than once in the same way to the same stimulus,* at that moment appeared simultaneously the germs of all human knowledge and faith. The moment a human being comes to know that his experiential knowledge must be incomplete knowledge, from the very conditions of its being knowledge at all, at that moment does he touch the highest level of knowledge and faith attainable by living beings. Agnosticism, mere disclaimer of absolute knowledge, can not be the loftiest attainable mental attitude. This must consist in knowing, partly at least, how and why your highest knowledge is limited and seemingly must ever remain so.

In conclusion, life from the biologist's standpoint is the sum total of the phenomena exhibited by myriads of natural objects called living because they present these phenomena. To understand any organism it must be studied *as a whole* and in all its relations. Taking man as a type, his life must be studied throughout the whole cycle of its existence on earth and in its relations to all other lives and things. Not only must the germ-cells, the chromosomes and all the rest be subjected to investigation as to their forms, vital activities and chemico-physical composition, but the whole gamut of his experiences, physical, intellectual and spiritual, must be likewise searched out, so far as it is possible for human minds to search.

No biologist can do much by working at the whole of biology thus viewed. But—and here is one of the centers of our position—he can toil in his particular corner with a mind full-illumined by the recognition that someone else must do the things he can not do because all must be done. He does not need to suppose the thing he is not doing is hardly worth doing.

of science as Lord Kelvin and Professor Arrhenius, that life like matter and force is eternal, must be taken hold of seriously as the best working hypothesis that can be made on the basis of the biological data available. This hypothesis will surely involve enormous difficulties, but some of the most difficult, one can foresee, will at least have the merit of being open to observational inquiry. Among the great difficulties will be that by "life" we must understand "organisms." We have no observational ground for postulating "organic substance" as anything else than the substance of which living beings are constituted.

This being so, the hypothesis would have to face at once the question, How numerous must the primal, eternally existent organisms be conceived to have been? But I am not adopting the hypothesis, not now, at any rate. I merely want to point out what seems a clearly possible alternative for the hypothesis of an actual beginning of life in time, which hypothesis seems to be growing less and less fruitful with the advance of experiential knowledge.

JOSIAH WILLARD GIBBS AND HIS RELATION TO MODERN SCIENCE. IV

By FIELDING H. GARRISON, M.D.

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The third stage of thermodynamics has for its point of departure Maxwell's observation that the second law is not a mathematical but an empirical or statistical truth, and his prediction that any attempt to deduce it from dynamic principles, such as Hamilton's principle, without introducing some element of probability, is foredoomed to failure.[127] "We have reason to believe of the second law," says Maxwell, "that though true, its truth is not of the same order as that of the first law," being an empirical generalization from the facts of nature in the first instance, while the molecular theory shows it to be "of the nature of a strong probability which, though it falls short of certainty by less than any assignable quantity, is not an absolute certainty." This statement of Maxwell's not only resumes the knowledge of his time, but has not been improved upon by later investigators, whose work shows that the truth of the second law is certain to the limit of human probability only. The theory of probabilities itself is exact as far as human observation goes. In 6,000 throws of dice, a particular facet will not necessarily turn up 1,000 times, but the probability of its doing so will be more nearly one sixth, the greater the number of throws. In the vital statistics of a great city the data of births, deaths, illegitimacy, etc., will be more nearly the same from week to week, the greater the population of the city; even the introduction of new dynamic factors, as seasonal change, epidemics, vaccination, antitoxin, etc., may alter particular effects but will not change the general tendency towards uniformity. Maxwell has observed that everything irregular, even the motion of a bit of paper falling to the ground, tends, in the long run, to become regular, and this is the rationale of testing the second law with respect to gases. In the kinetic theory of gases, the first scientific statement of which is due to Clausius, we assume a gas to be an assemblage of elastic spheres or molecules, flying in straight lines in all directions, with swift haphazard collisions and repulsions, like so many billiard balls. These, by Maxwell's calculations, will, if enclosed and left to themselves, gradually tend to an ultimate steady condition of perfectly equalized and permanently distributed velocities (*i. e.*, uniform temperature or thermal equilibrium) called "Maxwell's state." "This possible form of the final partition of

[127] *Nature*, 1877–8, XVII., 280.

energies," Maxwell claims, "is also the only form." At this point the work of Boltzmann becomes of central importance, especially on account of its profound influence on the later works of Gibbs. In Boltzmann's application of probabilities to Maxwell's problem, the starting point or initial stage of any sequence of events is called a "highly improbable one," because its certainty decreases the more the events proceed to some final or "most probable" state. For example, the blowing up of the *Maine* is to us a moral or mathematical certainty, but it may not be so æons hence, while its predisposing or exciting causes are even now "highly improbable" in that we know nothing positive about them. When a gas is brought into a new physical state, its initial stage is, in Boltzmann's argument, a highly improbable one from which the system of molecules will continually hasten towards successive states of greater probability until it finally attains the most probable one, or Maxwell's state of equilibrated partition of energy and thermal equilibrium. Maxwell's law of final distribution of velocities as determined by Boltzmann's probability coefficient is, therefore, a sufficient condition for thermal equilibrium, and Boltzmann found that the entropy of any state of gas molecules is proportional to the logarithm of the probability of its occurrence; or as Larmor puts it, the principle that the trend of an isolated system is towards states for which the entropy continually increases is analogous to the principle that the general trend of a system of molecules is through a succession of states whose intrinsic probability of occurrence continually increases. As a measure of the degree of variation of the gas molecules from Maxwell's state, Boltzmann introduces a function H such that, as the distribution of molecular velocities constantly tends toward the most probable distribution, H varies with the time and is found to be constantly diminishing in value. The necessary condition for thermal equilibrium is, therefore, that H should irreversibly attain a minimum value. Thus Boltzmann's "minimum theorem" becomes, like the Clausius doctrine of maximum entropy, a theorem of extreme probability,[128] or to quote the aphorism of Gibbs which Boltzmann chose as a motto for his *Gastheorie:* "The impossibility of an uncompensated decrease of entropy seems to be reduced to an improbability."[129] Applying similar reasoning to the material universe, Boltzmann finds that the following assumptions are possible: either the whole universe is in a highly improbable (*i. e.*, initial) state, or, as the facts of physical astronomy would seem to indicate, the part of it known to us is in a state of thermal equilibrium, with certain districts, such as the earth we live

[128] "It can never be proved from the equations of motions alone, that the minimum function H must always decrease. It can only be deduced from the laws of probability, that if the initial state is not specially arranged for a certain purpose, but haphazard governs freely the probability that H decreases is always greater than it increases." Boltzmann, *Nature*, 1894–5, LI., 414.

[129] *Tr. Connect. Acad.*, III., 229.

on, noticeably removed from this condition. The probability of the latter state of affairs is smaller, the further such a state is removed from thermal equilibrium, but it can be made as great as we please to assume the universe to be great. But there is necessary and sufficient probability that our earth as we know it is in its present state. By the second law (irreversible increase of entropy in natural processes) there is still greater probability that it tends to a final state of thermal equilibrium or death; and since the universe itself is so great, there is sufficient probability that other worlds than ours may deviate from thermal equilibrium. As a graphic exposition of this theory, which shows the vast scope of the second law of thermodynamics, a curve can be plotted with the variables H and the time as coordinates, to visualize what takes place in the universe. The H curve is shaped like a succession of inverted trees, the summits of which represent "the worlds where visible motion and life exist."[130] Physicists have found that the Maxwell-Boltzmann distribution of velocities is satisfactory for gases whose molecules move independently and at random; but when the molecules are supposed to be subject to one another's influence, it does not account for certain facts of nature such as the measured specific heats of gases or individual peculiarities of their spectra. In monatomic gases like argon, helium and mercury, the ratio of the specific heats will account for the three degrees of molecular freedom ascribed to them by the mathematical theory, but in the case of diatomic gases, like hydrogen or oxygen, the theory calls for six degrees of freedom, while experiment will account for only five. Boltzmann met these objections with frank or ironical admissions as to the ultimate inadequacy of all human hypotheses,[131] and although his theory is to some extent invalidated by facts like the above,[132] his subtle handling of molecular thermodynamics gives the physicist deeper insight into

[130] "Almost all these trees are extremely low, and have branches very nearly horizontal. Here H has nearly the minimum value. Only very few trees are higher, and have branches inclined to the axis of abscissæ, and the improbability of such a tree increases enormously with its height." Boltzmann, *Nature*, 1894–5, LI., 581.

[131] "Neither the theory of Gases nor any other physical theory can be quite a congruent account of facts, and I can not hope with Mr. Burbury that Mr. Bryan will be able to deduce all the phenomena of spectroscopy from the electromagnetic theory of light. Certainly, therefore, Hertz is right when he says: 'The rigour of science requires that we distinguish well the undraped figure of Nature itself from the gay-coloured vesture with which we clothe it at our pleasure.' But I think the predilection for nudity would be carried too far if we were to forego every hypothesis. Only we must not demand too much from hypotheses." Boltzmann, *Ibid.*, 413.

[132] The principal opponent of the Maxwell-Boltzmann partition of energies was Lord Kelvin in his "Nineteenth Century Clouds over the Dynamical Theory of Heat and Light." When asked what he had against it, he replied point-blank: "I don't think there is a single thing about it that is right" (*Science*, Jan. 3, 1908, p. 6).

such unusual phases of matter as radiation in rarefied gases, where the system has no temperature at all, because its internal motions have not settled down to a definite average. Helmholtz's dynamic proof of the second law assumes the existence of cyclic systems with reversible circular motions, like those of the gyroscope or the governor of a steam engine, in other words it assumes matter to be made of rotational or gyrostatic stresses in the ether. Gibbs's "Elementary Principles of Statical Mechanics" (1903)[133] is based upon no assumptions whatever except that the systems involved are mechanical, obeying the equations of motion of Lagrange and Hamilton. "One is building on insecure foundations," he says, "who rests his work on hypotheses concerning the constitution of matter," and his statistics deal, not with the behavior of gas molecules in isolated systems, but with large averages of vast ensembles of systems of the same kind (solid, liquid or gas), "differing in the configurations and velocities which they have at any given instant, and differing not merely infinitesimally, but it may be so as to embrace every conceivable combination of configuration and velocities." The problem is, given the distribution of these ensembles in phase (*i. e.*, in regard to configuration and velocities) at some one time, to find their distribution at any required time. To solve this problem Gibbs establishes a fundamental equation of statistical mechanics, which gives the rate of change of the systems in regard to distribution in phase. A particular case of this equation gives the condition for statistical equilibrium or permanent distribution in phase. Integration of the equation in the general case gives certain constants relating to the extent, density and probability of distribution of the systems in phase, which Gibbs interprets as the principles of conservation of "extension in phase," of "density in phase," and of "probability in phase." Boltzmann found that when the gas molecules have more than two degrees of freedom, the equations can not be integrated and further progress is impossible. He got around this difficulty by using Jacobi's "method of the last multiplier," which integrates the equations of motion. Gibbs found that the principle of "conservation of extension-in-phase," supplies such a Jacobian multiplier, "if we have the skill or good fortune (he says) to perceive that the multiplier will make the first member of the equation an exact differential." Boltzmann's probability coefficient is used as the index of the canonical distribution of ensembles, and when the exponent of this coefficient is zero, the latter becomes unity, producing a distribution in phase called "microcanonical," in which all the systems in the ensemble have the same energy, as in Maxwell's "state." After demonstrating the possibility of irreversible phenomena in the various ensembles, and after a careful study of their behavior when isolated, subjected to external forces or to

[133] "Yale Bicentennial Publications," 1903. Translated into German by Ernst Zermelo, Leipzig, 1905.

the spheres of one another's influence, Gibbs finds that the processes of statistical mechanics are to all human perception analogous to those of thermodynamics, the familiar formulæ of which appear, as Bumstead puts it, "almost spontaneously, as it seems from the consideration of purely mechanical systems." The differential equation relating to average values in the ensemble is found to correspond with the fundamental equation of thermodynamics; the modulus of distribution of ensembles turns out to be analogous to the temperature, while the average index of probability in phase is the analogue of the entropy with reversed sign, and being a minus quantity, is found to decrease just as entropy increases. Most of the objections filed against Gibbs's statistical demonstration, turn upon the fact that it is difficult, perhaps impossible, to apply the reversible dynamics of ideal, frictionless systems to the spontaneous irreversible phenomena of nature without making some physical assumptions. "Entropy," Burbury objects,[134] "may, for all that appears, either increase or diminish in a system which is dynamically reversible. This then can not be strictly applied to an irreversible process." Gibbs has met these objections fairly. "Our mathematical fictions,"[135] he says, to quote Burbury's paraphrase of his argument, "give us no information whether the distribution of phases is towards uniformity or away from it. Our experience with the real world, however, teaches us that it is towards uniformity." All actual mechanical systems are, as Gibbs pointed out long before, in reality thermodynamic,[136] and it seems odd that the critics who rejected Boltzmann's proof, because it did not agree with the facts of nature, should now, for a logical quibble, take exception to Gibbs's because it does. It has been predicted that future truth in physical science will often be found in the sixth place of decimals, for not everything in nature works out according to specifications. We can, if we choose, regard mathematics as a metaphysical diversion or employ it practically as a means of interpreting the physical facts of nature, empirically ascertained by man. In these matters, says Gibbs elsewhere, "Nature herself takes us by the hand and leads us along by easy steps as a mother teaches her child to walk,"[137] and he would have agreed with Langley that man may put questions to nature if he will, but is in no position to dictate her answers to them.[138] Nature seems *très femme* in this respect, especially in regard to mathematical fictions, that is, ideal or limiting cases devised by the finite mind of man.[139] Like any other human

[134] *Phil. Mag.*, 1904, 6. s., VIII., 44.

[135] *Ibid.*, 45.

[136] *Tr. Connect. Acad.*, III., 108.

[137] *Proc. Am. Ass. Adv. Sc.*, 1886, Salem, 1887, XXXV., 62.

[138] "Let us read Bacon again, and agree with him that we understand only what we have observed." S. P. Langley, *Science*, 1902, XV., p. 927.

[139] "Physical chemistry is not yet a quantitative science; it is a pseudo-quantitative science. There are all the outward signs of a quantitative science.

instrument of precision, our mathematical methods are but an approximation to the subtler aspects of nature, and it is only by eternal vigilance in regard to sources of human error that workers in physical science have put aside personal equation and infallibility and thus avoided what Rowland calls the "discontinuity" of the ordinary legal or cultivated mind.[140] "Gibbs has not sought to give a mechanical explanation of heat," says Professor Bumstead, "but has limited his task to demonstrating that such an explanation is possible. And this achievement forms a fitting culmination of his life's work."[141]

The naturalist Haeckel has explicitly denied the doctrine of universal increase of entropy[142] because, pointing as it does to the ultimate thermal death of different worlds, it conflicts with his monistic conception of the universe as a *perpetuum mobile,* consisting of infinite substance in eternal motion, without beginning and without end. Yet the cosmogony of Kant and Laplace, which Haeckel accepts, points to the same conclusion as well as to formative periods in the history of the solar and sidereal systems, in which entropy decreases, and energy, instead of dissipating, tends, after a maximum of degradation, to concentrate. Even possibilities of this kind put the second law on a lower plane of probability than the first as far as man is concerned, unless it be that the irreversible processes of nature are in reality cyclic, in which case we should have Nietzsche's "eternal return" of all things. But as Bumstead has so admirably said, "It is nearer the truth to base the doctrine of entropy upon the finite character of our perceptions than upon infinity of time."

In connection with the validity of the second law arises the important question of the extent of its application to animate nature and whether it is capable of reversal in vital processes. "The first law (conservation of energy) has been proven," says Ostwald, "with an exactness of 1:1,000 even for physiological combustion (including mechanical and psychical work performed)." The second law, whether in the Clausius form of increase of entropy, the Kelvin form of dissi-

We have formulas and tables; we make use of thermodynamics and the differential calculus; but this is for the most part a vain show. Long before we reach the point where the formula is to be tested experimentally we slip in a simplifying assumption: that the concentration of one component may be considered as a constant; that the heat of dilution is zero; that the solute may be treated in all cases as though it were an indifferent gas; that the concentration of the dissociated portion of a salt may be substituted for the total concentration; etc., etc. The result is that our calculations apply at best only to limiting or ideal cases, where an error in deducing the formula may be masked by errors in observation. Helmholtz did not do this, but Helmholtz is considered old-fashioned." W. D. Bancroft, *J. Phys. Chem.*, 1899, III., 604.

[140] H. A. Rowland, *Am. J. Sc.*, 1899, 4. s., VIII., 409.

[141] Bumstead, *Am. J. Sc.*, 1903, 4. s., XLI., 199.

[142] Haeckel, "The Riddle of The Universe," New York, 1900, 246–248.

pation of available energy, or the Gibbs-Helmholtz form of decrease of free energy, is assumed by recent physiologists to be characteristic of all spontaneous or metabolic processes, but both Helmholtz[143] and Kelvin[144] have doubted whether it is either necessary or sufficient for their production, while Maxwell[145] and Boltzmann[146] have asserted, what Gibbs's statistical researches seem to prove, that it is sometimes possible for entropy to decrease, that is for small isolated temporary violations of the second law to occur in any real body. Has animal or vegetable protoplasm ever the power ascribed to Maxwell's demon of reversing the thermodynamic order of nature, and directing physico-chemical forces? Such a demon, according to Lord Kelvin, might, through his superior intelligence or motor activity, render one half of a bar of metal glowing hot, while the other half remained icy cold. We have something analogous to this in certain diseases, as gangrene, aphasia, various forms of paralysis, the curious vasomotor and trophic disorders of the nervous system. Are these phenomena then of a thermodynamic nature? The animal body, Lord Kelvin thought, does not act like a thermodynamic engine, but "in a manner more nearly analogous to that of an electric motor working in virtue of energy supplied to it by a voltaic battery." Here, as Gibbs has shown in his theory of the chemical cells, the electromotive force would be identical with the free energy upon which the surface energies of the body must ultimately depend. Beyond these speculations we know nothing. Gibbs himself avowed his express disinclination to "explain the mysteries of nature," while Lord Kelvin, although affirming that physicists are bound "by the everlasting law of honor," to explain everything material upon physical principles, mystified friends and opponents alike by falling back upon a "vital principle" with "creative power" behind it as the *causa causans* of biological happenings. But the business of physics is with the material facts of the universe, and the invocation of creative power explains nothing and is subversive of determinism, or the relation of cause and effect in science. It may be that "man was born too late to ascertain final causes": he can only interpret the physical facts of his experience as he finds them and with the means at his disposal. An interesting attempt to explain the relation of life and mind to matter is found in the *energetische Weltanschauung*

[143] Helmholtz, *J. f. Math.*, v. 100, 137. Auerback, "Kanon der Physik.," 414.

[144] Kelvin, "Pop. Lect.," II., 199, 463, 464. See, also, the discussion in *Science*, 1903, N. S., XVIII., 138–146.

[145] Maxwell, *Nature*, 1877–8, XVII., 280.

[146] Der grosse Meister, dem auch diese Zeilen huldigen möchten, hat einst den Gedanken ausgesprochen, dass es in der Welt vielleicht Stellen giebt, wo die Entropie nicht wächst, sondern zunimmt," O. Chwolson. Boltzmann, "Festschr.," 1904, 33.

or energetic philosophy of Ostwald, which confessedly derives[147] from the thermodynamic argument of Gibbs, but should not be confused with the latter. Gibbs was concerned only with applying the laws of mechanics to physical chemistry. Compared with the case of nature, he says, thermodynamic systems are "of an ideal simplicity." To Ostwald, however, mind and matter are but forms of energy, which is the only thing eternal and immortal. "We can deal with measurable things, never with the unknown heart of nature," says Ostwald, yet his basic principle, energy, is to all intents and purposes identical with the eternal infinite substance of Spinoza, Goethe and Haeckel, "sive Deus, sive Natura naturans, sive Anima mundi appelletur." Matter, in Ostwald's scheme, is a group of energies in space; thought becomes a mode of energy involving evolution of heat, and "the problem of the connection between body and spirit belongs to the same series as the connection between chemical and electrical energy, which is treated in the theory of voltaic chains."[148] Falling in love, listening to a Beethoven symphony, identifying oneself with nature, are to Ostwald instances of dissipation of energy like any other.[149] Philosophy of this kind does not clear up the mystery of the relation of mind and matter. Descartes assumed that mind and matter exist apart as parallels, having no causal connection with each other. Spinoza held that neither can exist apart; indeed, he sometimes asserts their practical identity as different modes of the same eternal substance. But however intimately they may be associated, no scientist or philosopher has yet proven, whether in the body of man or in the origin of the universe, that one is either the cause or the effect of the other.

Assuming matter in mass to be ultimately made up of rotational, vortical or gyrostatic stresses or of energies, whether kinetic or potential, we encounter the formidable objection of Boltzmann, that it seems illogical, not to say unmechanical, to postulate motion as the primary idea with the moving thing as the derived one. Motion of what? we have a right to ask, since Ostwald disdains the ether of the physicists.[150] Matter, in the words of Sir Oliver Lodge, may be physically resolved

[147] "Wir wollen daher den Versuch wagen, eine Weltansicht ohne die Benutzung des Begriffs der Materie ausschliesslich aus energetischem Material aufzubauen . . . In der für die neuere Chemie grundlegenden Abhandlung von Willard Gibbs ist sogar dies Postulat praktisch in weitestem Umfange durchgeführt worden, allerdings ohne dass es ausdrücklich aufgestellt worden wäre." W. Ostwald, "Vorles. über Naturphilosophie," 165.

[148] *Monist*, 1907.

[149] W. Ostwald, "Individuality and Immortality," 44–46.

[150] "What the atom of each element is, whether it is a movement or a thing, or a vortex, or a point having inertia, all these questions are surrounded by profound darkness. I dare not use any less pedantic word than entity to designate the ether, for it would be an exaggeration of our knowledge to speak of it as a body, or even a substance," Lord Salisbury, "Rep. Brit. Ass. Adv. Sc.," 1894, 8.

"perhaps, into electricity, and that into some hitherto unimagined mode of motion of the ether," but no dynamic theory of the ether can resolve the ether into nothing. Assuming thought to be a mode of energy, the metaphysical argument that mind is at the bottom of motion seems more likely, in the last analysis, than that motion should be the cause of mind, for we can not conceive of a thing moving unless something moves it. Mind seems almost like an assemblage or complex of causes in itself, and is probably related to the brain as music to the violin. Destroy the violin and there will be an end of its music, but it needs other coefficients than the violin itself to get music out of it. Ostwald has himself admitted the force of Leibnitz's argument, that no mechanical explanation of cerebral action will ever account for the genesis of thought or the nature of consciousness: "Nihil in intellectu quod non prius in sensu, nisi intellectus ipse." Individual thinking may be the result of physico-chemical differences of structure or substance in the brain, but apart from the evidence of mind in the evolution and structure of the universe, different aspects of mind, as ideas, sensations and sentiments, seem to have an individual life of their own so far as man is concerned, and are "things" in the sense that, like external forces, they have profoundly influenced and determined the actions of individuals and of entire races. Human thought as a function of the human brain may disappear with man himself, but this does not annul the possibility of mind existing in manifold ways elsewhere in the universe. The electric waves of wireless telegraphy undoubtedly existed as motions in the air before man discovered and labeled them and may continue to exist and be apprehended in other spheres of thought when man is gone.

Man's capacity for error in these matters is determined by his anthropomorphic tendencies and by the fact that his intelligence is finite. Of the possibly infinite number of attributes of eternal substance postulated by Spinoza, the human mind can apprehend only two —thought and extension, and even here thought and sensation are the fundamental facts, while "all else is an inference and is probably essentially unlike what it appears to our senses." It seems impossible to break down the fact that there is no absolute causal connection between the two primary categories of Spinoza, who has anticipated most of modern psychology. For this reason such subjects as spiritualism, phrenology, faith-healing, telepathy have remained in the limbo of pseudo-science, although each has undoubtedly a shadowy reason for existence. It is as fair as any other hypothesis, then, to assume that man, in his higher mental or psychical activities, may, under certain conditions, be "freed from the galling yoke of space and time," or, in other words, released from the thraldom of the second law. Yet such an assumption, even if made by a Kelvin, would be, in our present state of knowledge, an expression of individual personal

belief, a literary or humane analogy, a leaning in the direction of the "fair humanities of old religion," but not a scientific fact. To fix our ideas for the material world we may accept the expanded statement of the second law which Ostwald gave in his Ingersoll lecture in 1906:[151] "Every known physical fact leads to the conclusion that diffusion or a homogeneous distribution of energy is the general aim of all happenings. . . . A partial concentration may be brought in a system, but only at the expense of greater dissipation, and the sum total is always an increase in dissipation."[152] Through the labors of Joule and Kelvin, Maxwell and Boltzmann, Gibbs and Helmholtz, Carnot's simple generalization about heat engines has been elevated to the dignity of an irrevocable law of nature, a principle of scientific determinism, giving one of the most complete and satisfactory answers that man can furnish to the great question: How does any event in the material universe come to pass? In Darwin's picture of nature the quiet woods and waters, so calm and peaceful on the surface, are in reality centers of "strange and cruel life," the struggle and turmoil of creatures continually preying upon each other, even trees and plants and the tiniest particles of animate bodies taking part in a definite, never-ending war for existence. But the stern law of life, whereby the strong war down the weak, loses all moral or human significance when seen as due, in the last analysis, to an inevitable tendency to dissipation of energy or as the resultant of a play of complex forces, which, through some principle of "least action," must inexorably flow from higher to lower potentials. As Spinoza pointed out long ago, Nature could not change these laws which flow from its very being, without ceasing to be itself, and the conclusion of physics and biology that Nature is never on the side of the weak becomes, as far as man is related to the material universe, identical with Spinoza's denial of final causes.

Apart from his work in mathematical physics, Gibbs made several important contributions to pure mathematics, notably in his theory of "dyadics," a variety of the multiple or matricular algebras which Benjamin Peirce classified as "linear associative." The tendency of his mind was always toward broad, general views and the simplifications that go with such an outlook, and here mention should be made of his charming address on multiple algebra and his innovation of vector analysis, a calculus designed to give the student of physics a clearer

[151] W. Ostwald, "Individuality and Immortality," Boston, 1906, 42.

[152] As a fundamental formula for all material happenings, analogous to the "world-formula" of Laplace, J G. Vogt proposes the following (*Polit. Anthrop. Rev.*, Leipzig, 1907–8, VI., 573): If *Pe* represent the positive or dissipational potential (*emissives Potential*) and *Pr* the negative or concentrational potential (*rezeptives Potential*) of any given set of forces, then $Pe + Pr = 0$ or $\int_n d\,Pe + \int_0^n d\,Pr = 0$. This is, however, only another restatement of Newton's Third Law of Motion, that action and reaction are equal and in opposite directions.

insight into such space relations as strains, twists, spins and rotational or irrotational movements in general. Maxwell, who once declared that he had been striving all his life to be freed from the yoke of the Cartesian coordinates, had already found such an instrument in the Hamiltonian quaternions, the application of which he brilliantly demonstrated in his great treatise on electricity and magnetism. Quaternions are elegant, consistent, concise and uniquely adapted to Euclidean space, but physicists have latterly found them artificial and unnatural to their science, because the square of the quaternionic vector becomes a negative quantity.[153] The Gibbsian vectors obviate this difficulty, and while seemingly uncouth, furnish a mode of attack more simple and direct and adaptable to space of any dimensions. Their capacity for interpreting space relations was amply tested by Gibbs in his five papers on the electromagnetic theory of light and his application of vectors to the calculation of orbits, since incorporated in recent German treatises on astronomy. The fact that vectors tend to displace the quaternionic analysis of Sir William Rowan Hamilton involved our author in a lengthy controversy with Hamilton's best interpreter, the ingenious and versatile Tait,[154] who looked upon Gibbs as "one of the retarders of quaternionic progress," defining his system as "a sort of hermaphrodite monster compounded of the notations of Hamilton and Grassmann." But Gibbs did not regard his method as strictly original; he was only concerned with its application in the task of teaching students; and when, after testing it by twenty years' experience in the class-room, he reluctantly consented to the publication of his lectures in full, the task was confided to one of his pupils, our author declining, with a characteristic touch of conscience, to have the work appear under his name or even to read the proof. In the controversy with Tait there is, as in most controversies, an amusing element of human nature. The name of Hamilton is undoubtedly one of the most illustrious in the history of science, and Tait and his adherents seemed to regard it as an impertinence and a desecration of his memory that any other

[153] "I have the highest admiration for the notion of a quaternion; but . . . as I consider the full moon far more beautiful than any moonlit view, so I regard the notion of a quaternion as far more beautiful than any of its applications. . . . I compare a quaternion formula to a pocket-map—a capital thing to put in one's pocket, but which for use must be unfolded: The formula, to be understood, must be translated into coordinates," Arthur Cayley, *Proc. Roy. Soc. Edinb.*, 1892–5, XX., 271. At the Southport meeting of the British Association in 1903, Professor Larmor, while admitting the extreme usefulness of the different methods of vector analysis, argued that their slow progress in physics was due to the lack of uniformity in definitions and notations, requiring that each system must be mastered separately before it can be applied. To which Professor Boltzmann not inaptly replied that the confusion might have been avoided, if Hamilton had adopted the notations of Grassmann in the first instance.

[154] *Nature*, 1891–3, *passim*.

system than quaternions should be proposed. "The ideas which flashed into the mind of Hamilton at the classic Brougham Bridge" became the occasion of a joined battle between the perfervid clan-loyalty of the Celt and the cool individualism of the Saxon; on one side,

> "The broad Scots tongue that flatters, scolds defies,
> The thick Scots wit that fells you like a mace,"

and on the other, the overconscientious, ethical arguments of a super-sensitive spirit, obviously nettled at certain rough pleasantries which were understood but not appreciated. In 1893 Heaviside, an English vectorist, reports "confusion in the quaternionic citadel: alarms and excursions and hurling of stones and pouring of water upon the invading hosts."[155] The vectorists were denounced as a "clique" and ridiculed especially for their lack of elegance, their alleged intellectual dishonesty and the fact that their pupils were "spoon-fed" upon mathematico-physical pap. But some of the notations held up to ridicule turned out to be things like Poisson's theorem or the difficult hydrodynamic problem "given the spin in a case of liquid motion to find the motion," which Helmholtz solved with one of his strokes of genius, and which Gibbs showed could be understood and interpreted by the average student without genius by a simple application of vectorial methods. The real point at issue in the controversy, the fundamental difference in the ideals of European and American education, lies here. Both have their relative advantages and defects, but the object of one has been to bring the best to the highest development, while the other is concerned with increasing the efficiency of the average man. One has been exclusive, aiming at the survival of the fittest; the other is democratic and inclusive, and aims, in Huxley's words, to make the greatest number fit to survive. The merits of the case are well summed up in Gibbs's final statement: "The notions which we use in vector analysis are those which he who reads between the lines will meet on every page of the greatest masters of analysis, or of those who have probed deepest the secrets of nature, the only diffrence being that the vector analyst, having regard for the weakness of the human intellect, does as the early painters who wrote beneath their pictures 'This is a tree.' 'This is a horse.'"[156] This view is in perfect accord with the recent trend of mathematical teaching, European or American, which is to emphasize the meaning and interpretation of equations and formulæ rather than their demonstrations or manipulation; in short, to substitute visualizing methods, the art of thinking straight and seeing clear, for what is conventional and scholastic. A Harvard professor is said to have told his students that the demonstration of a theorem is no evidence that it is understood, but the intelligent use of it is; and the object of such teaching as Gibbs's was to enable the

[155] *Ibid.*, 1892–3, XLVII., 534.
[156] *Ibid.*, 464.

student to see physical phenomena with the "clarity of vision" which Tait himself thought characteristic of the truly mathematical mind, and of which a good criterion is afforded in Helmholtz's unforgetable statement about Michael Faraday: "With wonderful sagacity and intellectual precision, Faraday performed on his brain the work of a great mathematician without using a single mathematical formula."[157]

At Yale Gibbs was esteemed an ideal teacher of physics, cordial, quick, helpful, willing to devote unlimited time to assist plodders and giving his students ample opportunity to learn "what may be regarded as known, what is guessed at, what a proof is and how far it goes." Of the qualities that make for distinction of mind and character he had the impersonal gift, "le don d'être né essentiellement impersonnel," which Renan thought highest of all, and which, fortunately for the advance of real knowledge, has been characteristic of most of the great leaders of science. He could build no wall of personal egotism between himself and the external facts, and "few could come in contact with this serene and impartial mind without feeling profoundly its influence in all his future studies of nature."[158] We know little of his life beyond the fact that he was a man of stoic fiber, who lived and worked alone. The countenance in the portraits expresses the Puritan austerity with lines that tell of mental stress and struggles with illness, but the man himself was "unassuming in manner, genial and kindly in his intercourse with his fellow men." "In the minds of those who knew him," concludes his biographer, "the greatness of his intellectual achievements will never overshadow the beauty and dignity of his life."[159]

American contributions to physics, from Franklin to Michelson, have been characterized by originality of invention and experiment. The work of Gibbs has a place apart as that of a mathematical theorist whose ideas have found wide application in the main current of modern thought, and his true position is best described in his own often-quoted estimate of his great predecessor, Clausius. "Such work as that of Clausius," he says, "is not measured by counting titles or pages. His true monument lies not on the shelves of libraries, but in the thoughts of men and the history of more than one science."[160] The general scientific reputation of Gibbs is of this kind, while in his chosen field of activity, the austere region of physics in which Newton and Lagrange, Hamilton and Jacobi are the leaders, his is assuredly the most distinguished American name.

[157] Helmholtz, Faraday Lecture, 1881.

[158] Bumstead, *Am. J. Sc.*, 1903, 4. s., XLI., 201.

[159] Bumstead, *loc. cit.*

[160] Gibbs, *Proc. Am. Acad. Arts and Sc.*, 1889, N. S., XVI., 465.

THE PROGRESS OF SCIENCE

THE DEATH OF SIMON NEWCOMB

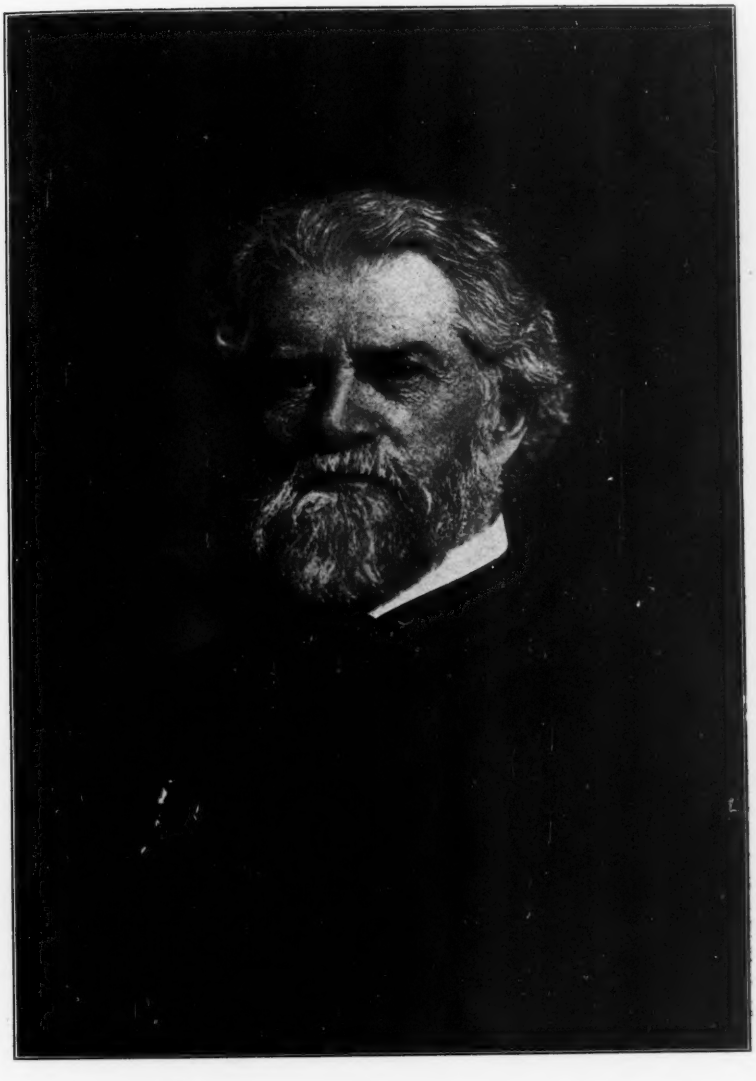
We have not had in America a great period of scientific productivity such as formed part of the Victorian era in Great Britain or followed the renaissance of the universities in Germany. Perhaps only in one science have we been in the position of leaders. In astronomy, thanks it may be to the endowment of observatories where research was not crowded by elementary teaching, we have done our share, or more than our share, for the advancement of science. Our great astronomer, who gave distinction to science in America, is now dead, and we mourn the loss of one whose place can not be filled.

Simon Newcomb was born on March 12, 1835, in a village of Nova Scotia, but was of New England descent from five generations of Simon Newcombs, as well as on the side of his mother. In his "Reminiscences of an Astronomer," published six years ago, there is an interesting account of his early life. His father was a school teacher who moved from village to village in accordance with the custom of the time. The child was apt at figures and had done arithmetic through cube root at the age of six and a half. He read with avidity the few books that came within reach, especially those concerned with science, but had no regular schooling or education in the ordinary sense. At the age of fourteen he was apprenticed as a boy of all work to an irregular practitioner in the hope that he might pick up some knowledge of medicine. This result not following, he ran away, worked his passage to Massachusetts in a sailing boat and found himself teaching in a country school in Maryland at the age of eighteen. A couple of years later, he became acquainted with Secretary Henry of the Smithsonian Institution, it may be through borrowing from the institution a copy of Laplace's "Mechanique Celeste," a knowledge of which he regarded as necessary for a computer. Such a position he soon afterwards obtained on the "Nautical Almanac," then conducted at Cambridge. He was at the same time able to enter Harvard University, where he studied under Professor Peirce and read in earnest the works of Laplace and La Grange.

Henceforth Newcomb's scientific career is a long record of sound and brilliant achievement. Beginning with work on the orbits of the asteroids he extended it to Uranus and Neptune and to other planets and to the moon. The mathematical genius required for work of this kind is of the highest type; many would regard Laplace as the greatest intellect that the world has produced, and in America he has had worthy successors in Newcomb and in Hill.

In 1861 Newcomb was appointed professor of mathematics in the navy, and in 1877 superintendent of the Nautical Almanac Office, a position which he held till he was relieved in 1897 at the age limit with the relative rank of rear-admiral. An appropriation to enable him to continue his work was made by the congress and later it was carried forward under the auspices of the Carnegie Institution to be ended only with his death. He declined the directorship of the Harvard Observatory, but accepted a professorship in the Johns Hopkins University in conjunction with his work at Washington.

In addition to his great work in celestial mechanics, Newcomb performed important services for astronomy and for science in many directions. One of



Simon Newcomb

these was in administration, and the national government owes much to his skill and wisdom. Another is in his numerous popular works and text-books. He was a master of clear thinking and good English—witness, for example, the series of papers on "The Stars," published in this journal in 1900. He was also the author of standard works on political economy and of a great number of articles, addresses and papers dealing with the problems of science over a very wide range.

It is needless to tell here of the honors conferred upon Newcomb. He was elected president of the American Association for the Advancement of Science at an early age. Honorary degrees and honorary membership in academies were heaped upon him. To be one of the eight foreign associates of the Paris Academy of Sciences is perhaps the highest recognition that can be given in the scientific world. It had not been awarded to an American since Franklin.

THE DARWIN COMMEMORATION AT CAMBRIDGE

THE centenary of the birth of Charles Darwin coinciding with the fiftieth anniversary of the publication of "The Origin of Species," has been adequately celebrated in the United States, as recounted in the April issue of this journal, which was itself a Darwin memorial number. It is, however, fitting that the principal commemoration should be held in Great Britain and at the University of Cambridge. Darwin, it is true, held no academic position and was not greatly influenced by his work as an undergraduate at Cambridge. He said later that his "time was wasted as far as his academic studies were concerned"; but he could also say: "the three years I spent at Cambridge were the most joyful of my happy life." The part often played by a college in the future life of a student through the friends and associations there formed is well illustrated in the case of Darwin. He became interested in collecting beetles through his cousin, W. Darwin Fox, also a student of Christ's College, and through Henslow, the eminent botanist, and it was through the latter that he was led to undertake the voyage on the *Beagle*. This was Darwin's true university course, and it is difficult to imagine just what he would have done in the world had it not been for the circum-

THE SECOND COURT OF CHRIST'S COLLEGE, in which were the rooms of Charles Darwin.

stances, which may be regarded as accidental, leading to this voyage. When we remember that his contemporaries, Huxley, Wallace and Hooker, were also led to their scientific work by a voyage of exploration, we must regard it as more than a mere incident in their lives.

It is truly remarkable that Christ's College, smaller than the average of our six hundred colleges and with no higher standards as far as the requirements of the curriculum go, can celebrate the tercentenary of the birth of Milton as well as the centenary of the birth of Darwin; that Tennyson and Darwin should have been fellow students, and that Newton, perhaps Darwin's only rival for scientific preeminence, should have been a member of the same university. Darwin's grandfather, Erasmus, was also a Cambridge student, and three of his sons are intimately connected with the university. Cambridge may well be proud of its great men and England of its great university; and this feeling we may share, remembering the descent of our academic institutions from the new Cambridge in New England.

An English university is certainly the place where a ceremonial such as the Darwin centenary has the most fit setting. To it came delegates from all parts of the world, some 230 in number, leaders in all departments of science and especially in the biological and evolutionary sciences. Lord Rayleigh, formerly professor of physics and now chancellor of the university, welcomed the guests to the Fitzwilliam Museum on the evening of June 22. On the following day, there was a presentation of addresses by the delegates in the Senate House. After the address of the chancellor speeches were made by Professor Oscar Hertwig, of Berlin; Professor Elié Metchnikoff, of Paris; Dr. Henry F. Osborn, of New York, and Sir E. Ray Lankester, of London. In concluding his remarks Dr. Osborn said that they, the delegates, naturalists and friends, desired to present to Christ's College, as a memorial of their visit, a portrait of Charles Darwin in bronze, the work of their countryman, William Couper, "a portrait which they trusted would convey to this and future generations of Cambridge students, some impression of the rugged simplicity as well as of the intellectual grandeur of the man they revered and honored."

On Wednesday evening the delegates and guests were entertained at a banquet held in the New Examination Hall, which was used for the first time for a public purpose. Among the speakers were the Right Hon. A. J. Balfour, Dr. Svante Arrhenius, Professor E. B. Poulton and Mr. William Erasmus Darwin, eldest son of Charles Darwin. On Thursday, the Rede lecture was given by Sir Archibald Geikie, president of the Royal Society, and honorary degrees were conferred on a number of delegates, including from America Professor Jacques Loeb, of the University of California; Secretary Charles D. Walcott, of the Smithsonian Institution and Professor Edmund B. Wilson, of Columbia University. During the celebration there was an exhibition held in Christ's College of pictures, books, manuscripts and other objects connected with Darwin, including the portraits by Richmand, Collier and Ouless, and the bronze bust by William Couper, of New York, which the American delegates presented to Christ's College.

THE WINNIPEG MEETING OF THE BRITISH ASSOCIATION

THE British Association for the Advancement of Science takes seriously its imperial functions. Four years ago it migrated to South Africa, and now, for the third time, it is about to hold a Canadian meeting and in the very center of the great dominion. The British Association has maintained its usefulness and prestige along the lines in which it was originally established. It is a great factor in the diffusion as well as in the advancement of science.

Its meetings are attended by the leading professional men of science and at the same time by large numbers of amateurs. The local members at each meeting are likely to exceed a thousand, and excellent arrangements are made for their instruction and entertainment. The social features are emphasized, so that there is opportunity for forming personal acquaintances and for those who are only interested in science to meet those most actively engaged in its advancement.

The Winnipeg meeting, which opens on August 25, will be presided over by the eminent Cambridge physicist, Professor J. J. Thomson, who succeeds Mr. Francis Darwin. Addresses of general interest will be given by the president and the presidents of the sections, and by Professor Herdman, Professor Tutton, Professor Dixon, Professor Poynting and others, and the sectional meetings are certain to have attractive programs. There will also be the usual extensive arrangements for garden parties, receptions and excursions. A visit to the Pacific coast, including Alaska and the Seattle Exposition, should be of unusual interest.

The Canadian railways offer a single fare, so the return trip from Montreal or Quebec to Winnipeg costs only thirty-six dollars. The council of the British Association has courteously voted to admit all members of the American Association for the Advancement of Science to membership for the meeting, waiving the entrance fee, and the American Association will hold no meeting this summer. A large number of Americans will doubtless take advantage of the generous invitation of their British colleagues and attend the Winnipeg meeting. It is a rare privilege that should be taken advantage of by all who find it possible.

SCIENTIFIC ITEMS

WE record with regret the death of Professor J. D. Cunningham, the anatomist of the University of Edinburgh, and of Dr. M. A. Brezina, the mineralogist of Vienna.

AMONG the honors awarded on the birthday of King Edward are knighthoods to Mr. Francis Galton, Professor J. Larmor, Mr. R. H. I. Palgrave and Professor T. E. Thorpe.—Mr. Orville Wright and Mr. Wilbur Wright were presented on June 19 with the gold medal authorized by congress, a medal on behalf of the state of Ohio and a medal on behalf of the city of Dayton.

DR. WILLIAM H. WELCH, professor of pathology in the Johns Hopkins University, has been elected president of the American Medical Association.—Professor E. W. Morley has been elected honorary president and Dr. W. H. Nichols acting president of the Seventh International Congress of Applied Chemistry, which has accepted the invitation extended by the congress through the president and the secretary of state, to meet in this country in 1912.

MR. JOHN D. ROCKEFELLER has made a further gift of $10,000,000 to the General Education Board. Its endowment is now $53,000,000. Mr. Rockefeller has authorized the board to distribute the principal as well as the income for educational purposes, should this at any future time appear to be advisable.